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The Impacts of the Climate Change Levy on Manufacturing: Evidence from Microdata

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Abstract
We estimate the impacts of the Climate Change Levy (CCL) on manufacturing plants using panel data from the UK production census. Our identification strategy builds on the comparison of outcomes between plants subject to the CCL and plants that were granted an 80% discount on the levy after joining a Climate Change Agreement (CCA). Exploiting exogenous variation in eligibility for CCA participation, we find that the CCL had a strong negative impact on energy intensity and electricity use. We cannot reject the hypothesis that the tax had no detrimental effects on economic performance and on plant exit.

Keywords: Climate policy, carbon tax, United Kingdom, manufacturing, impact assessment
JEL Classification: Q41, Q48, Q54, D21

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1 Introduction

The rise of climate policy on government agendas around the world has stirred a renewed interest in the optimal design of large-scale regulation of environmental externalities. Climate change – the "ultimate commons problem" (Stavins, 2011) – is caused by anthropogenic emissions of greenhouse gases (GHG) such as carbon dioxide (CO_2) and is expected to have severe ecological and economic consequences (IPCC, 2007). Mitigating climate change will require substantial abatement of GHG emissions from all core economic sectors (Pacala and Socolow, 2004). The choice of appropriate policy instruments for each of these sectors is essential for minimizing the overall economic costs of mitigation with given technologies (static efficiency), and for stimulating technological innovations that will further reduce mitigation costs in the future (dynamic efficiency). The relative performance of two such instruments to curb CO_2 emissions from the manufacturing sector is the focus of this paper.

Manufacturing is a major contributor to GHG emissions around the world.[1] Since most manufactured goods are tradable, there is a risk that regulated firms will lose international competitiveness, shed part of their labor force or even exit. These concerns have been fueling vehement opposition towards regulation in this sector and left their mark on the design of the policies implemented so far. Command-and-control policies have long been the predominant form of environmental regulation in the manufacturing sector, and their impacts have been studied extensively in the context of air pollution.[2] On theoretical grounds, economists

[1] Together with primary industry, the manufacturing sector accounts for almost 40% of GHG emissions worldwide (IEA, 2010).

[2] See, for example, the sizable empirical literature on the effects of command-and-control regulation of air pollution on emissions (Henderson, 1996; Greenstone, 2004), industrial activity (Becker and Henderson, 2000; Greenstone, 2002), plant births and deaths (Henderson, 1996; Levinson, 1996; List et al., 2003), plant-level productivity (e.g. Berman and Bui, 2001; Gray and Shadbegian, 2003), foreign direct investment (Hanna, 2010) and

have favored market-based instruments such as taxes and tradable permit schemes because they are more efficient in both the static and dynamic senses (e.g. Montgomery, 1972; Tietenberg, 1990; Milliman and Prince, 1989). However, empirical evidence on the impacts of market-based environmental regulation on manufacturing is scarce, especially when it comes to carbon emissions.[3] For example, the European Union Emissions Trading Scheme (EU ETS), the largest cap-and-trade system for carbon emissions worldwide, is overdue for a microeconometric evaluation. While carbon taxes have been implemented in various EU countries, their rigorous evaluation has proven difficult, be it for lack of suitable microdata or of a compelling identification strategy.[4]

This paper fills the void by analyzing the Climate Change Levy (CCL) package – the single most important climate change policy that the UK government has unilaterally imposed on the business sector so far. The package consists of an energy tax, the CCL, which added 15% to the energy bill of a typical UK business when it was introduced in 2001 (NAO, 2007). The government seeks to lighten the tax burden on energy intensive firms by offering an 80% discount on the tax rate to businesses that join a Climate Change Agreement (CCA) which obliges them to adopt a specific target for energy consumption or carbon emissions. These firms may also participate in emissions trading on the UK carbon market. Together, the CCL and CCAs were expected to contribute the lion's share of total carbon savings from the business sector under the UK Climate Change Programme between 2000 and 2010 (HM Government, 2006).

Given its scope and institutional context, the CCL package provides a unique opportunity to study the impacts of carbon pricing in an industrialized economy. We use longitudinal data on manufacturing plants to estimate the impact of the CCL on energy use and economic performance. Our strategy to identify the tax effect builds on the comparison of outcomes between fully-taxed CCL plants and CCA plants. This raises two issues. First, eligibility is not randomly assigned and eligible plants can decide voluntarily if they want to participate in a CCA.

market structure (Ryan, 2010).

[3]One reason for this is that most existing cap-and-trade programs do not cover manufacturing emissions in significant ways. The RECLAIM program for NO_X emissions in California is an exception (Fowlie et al., 2011).

[4]See Bjorner and Jensen (2002) for an early microeconometric evaluation of industrial energy taxes in Denmark.

We thus expect a fair amount of self selection into CCAs to be present, which might cause bias in simple least-squares or fixed-effects estimates. To address this problem, we adopt an instrumental variable framework that exploits exogenous variation in the initial CCA eligibility rules. Specifically, since eligibility for the discount was tied to whether or not a facility emitted any pollutants subject to pre-existing environmental regulation – the Pollution Prevention and Control (PPC) Act – we can use this information to instrument for the tax rate. The second issue is that firms in the control group were not only entitled to a tax discount, but they also faced a reduction target for energy consumption or carbon emissions. To the extent that this target places a binding constraint on the plant's production choices, we recover a lower bound on the full price effect of the tax differential between the two groups of plants.

We find robust evidence that the CCL had a strong negative impact on energy intensity, particularly at larger and more energy intensive plants. An analysis of fuel choices at the plant level reveals that this effect is mainly driven by a reduction in electricity use and translates into a negative impact on CO_2 emissions. In contrast, we do not find any statistically significant impacts of the tax on employment, gross output or total factor productivity (TFP). This means that worries about adverse effects of the CCL on economic performance of surviving plants are unsubstantiated. Looking at extensive-margin adjustment, we find no evidence that the CCL accelerated plant exit. We conclude that, had the CCL been implemented at full rate for all businesses, further cuts in energy use and carbon emissions could have been achieved without jeopardizing competitiveness.

Our study extends a small number of previous assessments of the CCL which have struggled with two main problems. First, it has proven difficult to establish generally accepted baselines against which to measure progress of firms in CCAs towards their targets. Second, aggregate energy data used in previous analyses are ill-suited to identify the causal impact of the CCL separately from that of unrelated changes in the economic environment, including other policies that were introduced concurrently under the UK Climate Change Programme. To circumvent these problems, we compile comprehensive microdata from restricted-access and other sources, and adopt a research design that identifies the *causal* impact of the CCL relative to that of the CCA plus emissions trading. More generally, our study provides much-needed empirical evidence on the impacts of large-scale regulation aimed at pricing pollution. It does so in the context of climate change – an area where regulatory stringency is bound to increase in the near future – and with a focus on manufacturing, the principal engine of growth in the emerging economies and still a cornerstone of employment in post-industrial economies.

The remainder of the paper is structured as follows. Section 2 describes the CCL package in detail and reviews previous research on the tax. Section 3 describes the research design and econometric framework. Section 4 describes the data sources and summarizes the dataset used for the analysis. Section 5 reports the main results and presents several robustness checks. Section 6 examines heterogeneous impacts, aggregate effects and estimates the impact of the CCL on exit. Section 7 concludes.

2 Background

2.1 The Climate Change Levy and Climate Change Agreements

Since the 1990s the UK has adopted a series of increasingly ambitious targets for climate policy. In addition to a 12.5% reduction of GHG emissions from 1990 levels to be achieved under the Kyoto Protocol, the Blair administration promised to reduce CO_2 emissions by 19% until 2010 and by 60% until 2050. With the passing into law of the Climate Change Bill in November 2008, the commitment to reduce GHG emissions in the UK by at least 80% until 2050 has become legally binding.[5] The CCL and CCAs constitute the single-most important policy package that the UK has implemented unilaterally in order to achieve these goals.[6] By official estimates, combined carbon savings from the CCL and CCAs would amount to 6.6 megatonnes of carbon (MtC) in 2010, making it the top contributor towards a total reduction of 20.8 MtC projected by the UK Climate Change Programme 2006 (HM Government, 2006).

The CCL is a per unit tax payable at the time of supply to industrial and commercial users of energy. It was first announced in March 1999 and came into effect in April 2001. Taxed fuels include coal, gas, electricity, and non-transport liquefied petroleum gas (LPG). For each

[5] It is permissible, however, that part of this reduction may be achieved through action abroad.
[6] Only the second phase of the EU ETS is expected to bring larger carbon savings.

Table 1: Taxation of energy and carbon content by fuel type

Fuel type	Tax rate $\left[\frac{\text{pence}}{\text{kWh}}\right]$	Fuel price $\left[\frac{\text{pence}}{\text{kWh}}\right]$	Implicit carbon tax $\left[\frac{£}{\text{tC}}\right]$
Electricity	0.43	4.25	31
Coal	0.15	2.46	16
Gas	0.15	0.91	30
LPG	0.07	0.85	22

Notes: Average fuel prices in 2001 based on QFI sample.
Carbon prices taken from Pearce (2006).

fuel type subject to the CCL, Table 1 displays the tax rates per kilowatt hour (kWh), the average energy price paid by manufacturing plants in 2001 and the implicit carbon tax. Energy tax rates vary substantially across fuel types, ranging from 6.1% on coal to 16.5% on natural gas.[7]

While the tax establishes a meaningful price incentive for energy conservation overall, it is immediately seen that carbon contained in gas and electricity is taxed at almost twice the rate as carbon contained in coal.[8] Other fuel types were tax-exempt precisely because of their low carbon content, such as electricity generated from renewable sources and from combined heat and power. Hence, rather than a pure carbon tax the CCL is a tax on energy with non-uniform rates, shaped by a mixed bag of fiscal and regulatory goals.

Revenue from the CCL is, to a large extent, recycled back into industry in the form of a 0.3% reduction of the employers' share of National Insurance Contributions (NIC). A small part of the revenues are diverted to the Carbon Trust, an institution set up by the government to foster research and development into energy efficiency schemes and renewable energy resources.

Similar to other European governments that had introduced energy taxes during the 1990s, the UK government set up a scheme of negotiated agreements, the CCAs, in order to mitigate possible adverse effects of the CCL on the competitiveness of energy intensive industries. By participating in a CCA, facilities in certain energy intensive sectors can reduce their tax liability by 80% provided that they adopt a binding target on their energy use or carbon emissions.

[7]Tax rates were constant from 2001 until 2006 and adjusted for inflation only in April 2007.

[8]David Pearce (2006) attributed this perverse effect to historical ties between the governing Labour Party and the coal industry, which had suffered from the "dash for gas" over the 1990s and successfully lobbied for a lower tax rate on coal. Mineral oil was exempt from the tax because it was already covered by the rather unpopular 'Fuel Duty Escalator', a policy of automatic increases in the taxes on diesel and gasoline. Residential energy use was not taxed for fear of a possible regressive effect (Pearce, 2006).

Targets were negotiated at two levels. In an 'umbrella agreement', the sector association and the government – represented by the Department for Environment, Food, and Rural Affairs (DEFRA)[9] – agreed upon a sector-wide target for energy use or carbon emissions in 2010 and on interim targets for each two-year 'milestone period' (i.e. 2002, 2004, 2006, 2008). At a lower level, 'underlying agreements' stipulate a specific reduction to be achieved by a 'target unit', i.e. a facility or group of facilities in a sector with an umbrella agreement. Targets were defined either in absolute terms or relative to output. At the end of each milestone period, the sector associations reported to DEFRA whether the sector-wide target had been met. Only if a sector-wide target had been missed did DEFRA verify compliance at the target unit level. A facility that was found in non-compliance was not re-certified for the reduced rate in the following milestone period. If the facility missed the 2010 target it faced the threat to repay all rebates on the levy it had accumulated in previous periods.

DEFRA originally negotiated 44 umbrella agreements with different industrial sectors, including the ten most energy intensive ones (aluminium, cement, ceramics, chemicals, food and drink, foundries, glass, non-ferrous metals, paper, and steel). Sector definitions used in the umbrella agreements rarely coincide with common economic classification systems. While most sector associations have chosen relative targets for energy, absolute targets were negotiated for the aerospace, steel, supermarkets and wall coverings sectors. Carbon targets were negotiated for the aluminium and packaging (including metal packaging) sectors.

While the primary objective of both the CCL and the CCAs is to enhance the efficiency of energy use in the business sector, the two instruments represent fundamentally different approaches. The levy provides a price signal at roughly 15% of energy prices faced by the typical business in 2001 (NAO, 2007). If energy demand is price sensitive, the increased relative price of energy should lead to improvements in energy efficiency and – in the absence of a strong rebound effect or exogenous increases in economic activity – to a reduction in energy use. In terms of CO_2 emissions, even the negative effect of an absolute reduction in energy use could be offset by a shift towards more carbon-intensive fuels.

In contrast, the CCA combines a very diluted price signal (approximately 3% of energy

[9]Since 2008 CCAs are administered by the newly created Department of Energy and Climate Change (DECC).

prices faced by the typical business) with quantity regulation, mostly in the form of efficiency targets. This target affects the plant only if it places a binding constraint on the dynamic trajectory of energy use during the remaining economic lifetime of the plant. If this is not the case, the plant faces weaker incentives for energy conservation than it would under the full tax rate. Since most targets are specified in terms of energy units rather than carbon emissions, there is no guarantee that even a stringent energy target leads to reductions in GHG emissions.

2.2 How stringent are the targets negotiated in the CCAs?

In theory, a government with perfect information about the firm's abatement cost can choose a tax discount and reduction targets so as to induce at least as much abatement as under the full tax rate (Smith and Swierzbinski, 2007). In reality, however, the government is unlikely to have perfect information about firm-specific abatement cost, especially if firms worry that sharing this information with the government weakens their bargaining position in the target negotiations. What is more, the government is unlikely to drive a hard bargain because of concerns about adverse effects on competitiveness and exacerbating distortions in marginal abatement cost (de Muizon and Glachant, 2003; Smith and Swierzbinski, 2007).

In fact, a closer inspection of the negotiation, monitoring and enforcement of CCA targets suggests that, as a rule, they were not placing any binding constraints on firm behavior. Officially, sector targets were set in such a way that they would close 60% of the average gap in energy efficiency between a "business as usual" (BAU) and an "all cost effective" scenario. The latter scenario assumed that firms implemented all efficiency enhancing measures that were cost effective without placing restrictions on the availability of management time and capital.[10] For the BAU scenario, the government assumed that average energy efficiency in energy intensive sectors improved by 4.8% between 2000 and 2010. This number was at the low end of available estimates. For example, the European Commission estimated a 9.5% improvement for all UK industry during the same period (DG Transport and Energy, 1999), and the Department of Trade and Industry (DTI, 2000) expected an improvement of 11.5%.[11] Since the average

[10]These measures included operational changes, low-cost retro-fit measures, major plant investments, and combined heat and power generation (CHP)(AEAT, 2001).

[11]This accounts for the effect of the CCL alone without CCAs.

11% reduction target to be achieved in sectors with a CCA target falls well into this range of BAU estimates, some observers were concerned that the government "double counted" carbon savings from the CCA scheme (ACE, 2005).

The fact that CCA sectors massively overcomplied with their 2010 targets did not help to dissipate such concerns. Combined annual carbon savings in all CCA sectors were substantially larger than the 2010 target throughout the first three milestone periods. For the first milestone period, CCA sectors reported savings of 4.5 MtC – almost twice the target amount of 2.5 MtC to be achieved by 2010. Most of this (2.6 MtC) was due to a dramatic decline in steel production. But even without steel and three other sectors that adopted absolute targets there was substantial overcompliance, with estimated carbon savings of 3 MtC (3.9 MtC and 4.3 MtC, respectively, in subsequent milestone periods; see NAO, 2007).

Parallel to overcompliance at the sector level, a consistently high proportion of target units were re-certified for the reduced tax rate. This proportion rose from 88% in the first period to 98% and 99% in the second and third target periods, respectively (AEAT, 2004; 2005; 2007). Most CCA participants complied with their targets, and those who did not could meet their targets by buying emission allowances on the UK Emissions Trading Scheme (UK ETS), a market for carbon permits that was operational between 2002 and 2006. Due to significant oversupply of carbon credits, allowance prices remained below the implicit carbon tax rates given in Table 1.[12] In fact, the lower bound on compliance cost is zero because a considerable amount of facilities that missed their target were re-certified for subsequent milestone periods thanks to the sector as a whole meeting its target. This was true, for example, of approximately 250 non-compliant target units when the 2004 milestone was reached (NAO, 2007).

In addition, a large degree of flexibility was built into the target negotiations both prior and subsequent to the compliance review. Target units were allowed to call upon several 'risk management tools' that made it easier to meet their targets. Ex-post adjustments to targets could be made to reflect a more energy intensive product mix, declining output (if minimum energy use was spread over fewer units), or 'relevant constraints' arising from other types of

[12] Smith and Swierzbinski (2007) present data showing that the allowance price fluctuated between £7 and £15 per ton of carbon (£2 and £4 per ton of CO_2 equivalent) for most of the period. Activity on the UK allowance market increased in March 2003 and March 2005 when firms participating in CCAs bought allowances to meet their interim targets. Yet the demand for permits was not large enough to put upward pressure on the price.

regulation. In some sectors, performance was measured against a 'tolerance band' in lieu of a fixed target. These risk management tools had to be approved by the government, and some of them were discontinued in later periods (NAO, 2007).

Ex-ante flexibility was ensured by permitting each sector to choose its own baseline year. More than two thirds of all sectors chose baseline years of 1999 or earlier, in some cases going as far back as 1990 (NAO, 2007). This means that carbon savings that had occurred before the policy package was implemented or even announced could be counted towards the target achievement. In some instances, fast growing companies that belonged to a sector with an absolute target successfully bargained for a relative target (and vice versa) as this made it easier to achieve compliance.

In sum, there is ample evidence that negotiated CCA targets are unlikely to have placed binding constraints on energy use by CCA companies.

2.3 Previous evaluations of the CCL package

Several evaluations of the CCL package were conducted at different stages of its implementation. In the 2000 Regulatory Impact Assessment, the government projected that the CCL instrument alone would achieve carbon savings of at least 2 MtC in 2010 against BAU projections (HMCE, 2000). This estimate was based on a model of business energy use maintained by the Department of Trade and Industry (DTI).[13] An official interim evaluation was commissioned at the end of the second commitment period. The study's main finding is a reduction in energy demand by the service and public sectors (which excludes manufacturing) following the announcement of the CCL package in March 1999 (Cambridge Econometrics, 2005). The authors identify this "announcement effect" as a structural break in an error correction model of quarterly energy demand. They argue that the effect is permanent rather than transitory (see Agnolucci et al., 2004, for more details).

Other studies have used a macroeconometric model of the UK economy (MDM-E3) to forecast business energy use under different versions of the CCL package. In line with the evidence presented in the previous subsection, this model predicts "that the energy (and therefore

[13]Total carbon emissions from the business sector in 2000 were estimated at 60.3 MtC (NAO, 2007).

carbon) saving and energy-efficiency targets would have been met without the CCAs" (Cambridge Econometrics, 2005, p. 7).[14] Moreover, model simulations of the CCL package give rise to much smaller carbon savings than those AEAT (2004) computed for the first milestone period. Ekins and Etheridge (2006) conclude from this that "the CCL package as implemented [...] achieved a greater carbon reduction than a no-rebate CCL would have done by itself" (p.2079). They attribute excess carbon savings to the possibility that managers became aware of more cost-effective efficiency enhancement projects as they started to benchmark their energy use. Barker et al. (2007) simulate the impact of the CCAs on macroeconomic outcome variables such as output, employment and industrial energy demand. In their exercise, a large effect of the CCAs on sectoral energy demand – averaging a 9.1% reduction in sectoral energy use by 2010 – is built into the model rather than estimated.

Overall, these assessments of the CCL package highlight two fundamental difficulties in policy evaluation, namely (i) to determine a valid baseline against which to measure the impact of a policy and (ii) to attribute any measured impact to this policy in a causal fashion. Since the studies use simulated trajectories of energy use as a baseline against which to measure the impact of the CCL package, their results critically depend on those counterfactual baselines. By definition, counterfactual scenarios are not observable, hence the evaluation results are subject to a large degree of uncertainty. When simulations are done using a macroeconometric model, this uncertainty derives not only from compounded error in the estimation of the underlying parameters but also from changes in the economic environment and from structural changes in the parameters (Lucas critique).

Furthermore, time series data aggregated at the sector level can hardly provide conclusive evidence in terms of discerning the effects of the policy from other concurrent events in a dynamically changing economic and political environment. When the CCL package was introduced, energy markets in the UK had been undergoing important changes that entailed significant and prolonged adjustments to prices, notably declining electricity prices and increasing prices of gas and coal. The levy interacted with a number of pre-existing other taxes

[14] With the exception of the "other industry" sector, which comprises all manufacturing other than basic metals, mineral products and chemicals, the authors find that the targets would have been met at the reduced rate or even without any CCL at all.

in the business sector, such as National Insurance Contributions and the Fuel Duty Escalator. Not least, with the Enhanced Capital Allowance and Carbon Trust energy audits, other energy efficiency enhancing measures were introduced simultaneously.

Ours is the first evaluation of the Climate Change Levy package to use longitudinal business microdata. Our approach addresses the baseline problem by comparing changes in actual firm behavior under two types of policy regimes, thus purging the effect of aggregate shocks. Moreover, we identify the causal effect of the tax by exploiting exogenous variation in the eligibility rules for the tax rebate. The next section explains our research design in detail.

3 Research design

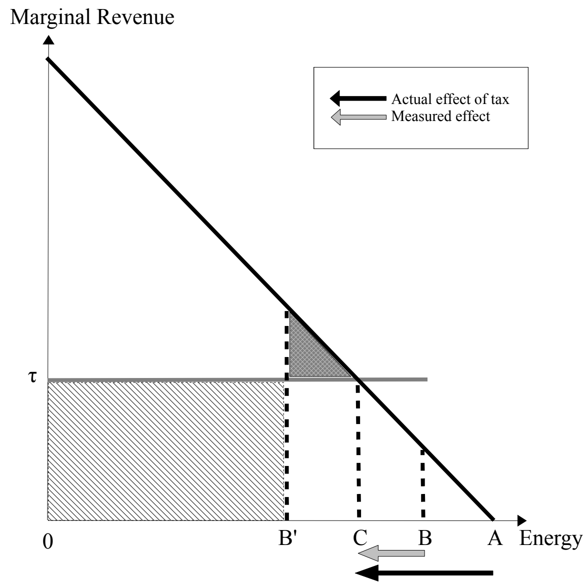
We seek to estimate the effect of the CCL by comparing plants that pay the full tax rate with plants that pay just 20% of the tax by virtue of being in a CCA. We consider the estimation equation

$$y_{it} = \alpha T_{it} + x'_{it}\beta + \xi_t + \eta_i + \varepsilon_{it} \tag{1}$$

where y_{it} is an outcome variable (for expositional purposes, think of energy use), T_{it} is the treatment dummy indicating that a plant pays the full rate of the tax, x_{it} is a vector of exogenous covariates (including a constant), ξ_t and η_i are unobserved year and plant effects, respectively, and ε_{it} is a random disturbance term. Three fundamental issues need to be addressed. First, while the CCA plants in the control group receive a tax discount they are also subject to an energy consumption or efficiency target which might affect their choices. Second, participation in a CCA is voluntary but not every plant is eligible. This potentially creates a selection endogeneity in the control group. Finally, the tax might have heterogeneous impacts among the group of treated plants.

Consistent estimation of equation (1) recovers the full effect of the CCL if – as previous research has suggested – CCA targets did not impose binding constraints on firm behavior. If the converse is true, the estimated α falls short of the true price effect as control plants choose lower-than-optimal levels of energy so as to comply with their CCA target. Hence, the estimated parameter α can be regarded as a conservative estimate of the impact of the CCL.

Figure 1: Target vs. Tax Effect



Notes: The plant's optimal energy consumption in the absence of policies is at point A. Given a tax τ, the optimal consumption drops to point C. If the target set by a Climate Change Agreement (CCA) is at an intermediate point such as B, comparing CCA and non CCA plants provides a meaningful lower bound for the impact of the tax. On the other hand, if the target is at B' we would not be able to identify the decrease in energy consumption from A to C due to the tax. For simplicity, we have drawn the reduced tax rate to coincide with the horizontal axis.

Figure 1 illustrates this point.[15]

In order to estimate α consistently, one needs to address the issue of non-random selection of plants into the control group. Section 4.2 below presents ample evidence of selection on observables, as CCA plants are, on average, older, larger and more energy intensive. Clearly, plants using large amounts of energy receive a larger absolute discount on their CCL liability which gives them a stronger incentive to join a CCA. In turn, as there are fixed costs of participating in a CCA, plants with low levels of energy use may find it more profitable *not* to join.[16] This is illustrated in Figure 2a. Controlling for observables may not solve the selection problem if there are unobservable differences in marginal abatement cost. As shown in Figure 2b, even for two plants that initially use the same quantity of energy, the one with the steeper marginal abatement cost schedule has a stronger incentive to join the CCA.

[15]The stringency of CCA targets – though relevant for the interpretation of the estimated effect as a lower bound on the full tax effect – does not affect the consistency of the estimation procedure. For example, if the targets were more stringent than the full-rate tax then our method would lead to a negative coefficient on CCA participation. This would still be a lower bound on the tax effect, albeit not a meaningful one.

[16]In personal communications, representatives of CCA sector associations pointed out multiple sources of fixed costs to us. The main cost drivers are payments to consultants or staff for doing the necessary energy accounting and administrative work as well as administrative fees charged by the sector associations.

Thanks to having panel data we can control for selection based on time-invariant unobserved heterogeneity η_i across plants by taking first differences of equation (1). This yields[17]

$$\Delta y_{it} = \alpha \Delta T_{it} + \Delta x_{it}' \beta + \Delta \xi_t + \Delta \varepsilon_{it}. \tag{2}$$

Least-squares estimation of equation (2) provides an unbiased estimate of the treatment effect α if $\Delta\varepsilon_{it}$ – the short-term deviation from a plant's idiosyncratic trend in energy consumption – is exogenous to the decision to join a CCA. This is not true if plants take into account their future energy consumption when deciding on CCA participation. Plants expecting to expand their energy consumption may perceive the CCA target as a binding constraint and therefore rather not join a CCA, whereas plants that expect a reduction in consumption will take the opportunity to reduce their tax liability provided that the (fixed) cost of joining the CCA is not too large. As a result, plants might select themselves into treatment and control groups based on time-varying unobserved shocks to the outcome variable, causing bias in the estimate of α.

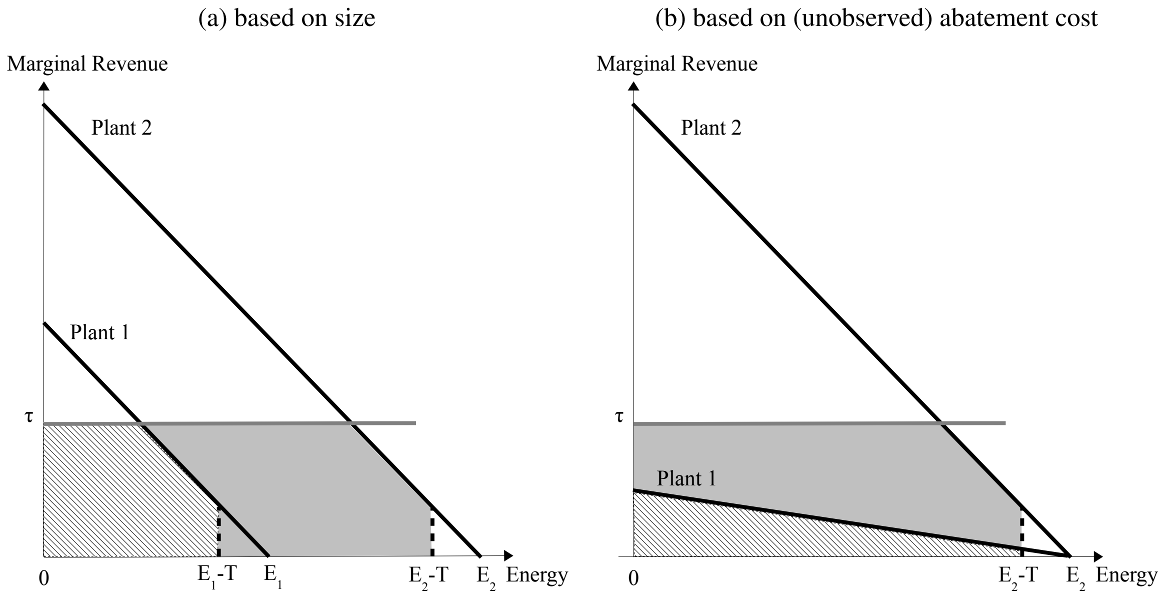
To address this issue we propose an instrumental variable approach based on eligibility rules for CCA participation. As explained above, the government intended to base eligibility upon energy intensity, yet in practice granted eligibility to all qualifying part A activities under the PPC Act. An indicator variable Z of whether or not a facility carries out such an activity should thus be a good predictor of CCA participation. When ΔZ is used as an instrument for ΔT in equation (2), the identifying assumption is that PPC part A coverage is orthogonal to shocks $\Delta\varepsilon_{it}$ that occurred after 2000.[18]

For a better grasp of the intuition behind this instrument consider the glass industry. Both the production and the recycling of glass containers are very energy intensive processes. However, since only the former is pollution intensive, glass container recycling was not eligible

[17] In our data we face the practical issue that some smaller plants are not sampled consecutively. In order not to throw away information on those plants we define the dependent variable in equation (2) as $\Delta y_{it} = y_{it} - y_{it-1}$ for $t \leq 2000$ and $\Delta y_{it} \equiv y_{it} - y_{i2000}$ for $t > 2000$ and transform the RHS accordingly. We also control for trends at the level of 3-digit sector and region. See Appendix A for details.

[18] The exclusion restriction also rules out the possibility that public disclosure under EPER had a direct effect on the outcome variable. While this assumption is untestable, we are not aware of any evidence that EPER reporting requirements affected firm behavior in the UK. In the context of the US Toxic Pollution Inventory, studies have found no significant effects of public disclosure rules alone on pollution abatement, stock market returns or housing prices (Bui and Mayer, 2003; Bui, 2005). Moreover, the fact that pollution emissions in 2001 were published only in 2004 precludes any direct effects operating through the demand side.

Figure 2: Selection into Climate Change Agreements



Notes: Consider two plants that are given the same absolute energy reduction target T. In sub-figure (a), marginal revenue cost curves are identical except for the fact that plant 1 uses less energy than plant 2. Upon joining a CCA, plant 1 saves the striped area in taxes and abatement cost whereas plant 2 saves the sum of the striped and grey areas. It is easy to control for size, but unobservable factors such as the slope of the marginal revenue cost curve also influence the incentives to join a CCA. In sub-figure (b) plant 1 is assumed to differ not in size but in abatement technology. Cheaper abatement options make CCA participation less attractive for plant 1 than for plant 2.

for CCA participation until the eligibility rules were revised in 2006. Similarly, the eligibility rules for the British Apparel and Textile Confederation were amended in 2006 to include low-pollution, high-energy users that had previously been excluded from CCA participation. This institutional 'glitch' provides us with exogenous variation in the probability of treatment.

Since the treatment variable T is binary and the instrumental variable Z is based on eligibility, the coefficient estimate $\hat{\alpha}$ recovers the average treatment effect on treated plants (ATT). A possible complication arises from the existence of reporting thresholds in the database that we use to construct Z. Since these thresholds were irrelevant when determining CCA eligibility, we may miss eligible plants whose emissions remain below the EPER reporting thresholds set for each of the 50 pollutants covered under PPC part A. In Appendix B we show that this shortcoming does not bias the coefficient unless the ATT parameter differs between plants above and below reporting thresholds, conditional on observable characteristics.

Econometrically, we perform a two-stage least squares estimation where the first stage is a regression of the treatment on the instrumental variable

$$\Delta T_{it} = \tilde{\alpha}\Delta Z_{it} + \Delta x'_{it}\tilde{\beta} + \xi_t + \Delta\tilde{\varepsilon}_{it} \tag{3}$$

and the second stage is a regression of outcome variables on predicted treatment indicators from the first stage

$$\Delta y_{it} \quad = \quad \alpha\Delta\hat{T}_{it} + \Delta x'_{it}\beta + \xi_t + \Delta\varepsilon_{it}. \tag{4}$$

We also consider a reduced-form or "intent-to-treat" regression of the outcome on the instrument variable

$$\Delta y_{it} = \alpha\Delta Z_{it} + \Delta x'_{it}\beta + \xi_t + \Delta\varepsilon_{it}. \tag{5}$$

4 Data

The compilation of a dataset suitable for the microeconometric evaluation of the CCL required a major effort in terms of data collection, cleaning and matching. The result is a unique dataset that matches publicly available information on CCA participation and EPER coverage to production data from two confidential business datasets.

4.1 Data sources

The core dataset is the Annual Respondents Database (ARD) which is maintained by the Office for National Statistics (ONS) and can be accessed by approved researchers through its Virtual Microdata Laboratory. The ARD is an annual production survey that covers about 10,000 plants in the manufacturing sector.[19] During the sample period, all plants with 250 employees or more (in some industries: 100 or more) had to report annually whereas smaller plants were

[19]Here and in the remainder of the paper a "plant" corresponds to an ARD reporting unit. This is the lowest aggregation level for which production data is available. In 70% of all cases a reporting unit is indeed a business unit at a single mailing address – a 'local unit'. Larger business units are allowed to report on several local units combined so as to reduce compliance costs. The information linking local units to reporting units is obtained from the Interdepartmental Business Register (IDBR), which in addition provides information on plant births and deaths as well as on employment, location and industry. For more details see Criscuolo et al. (2003).

included on a random basis (Barnes and Martin, 2002). The ARD comprises a wide range of economic characteristics of the plant, including turnover, value added, total purchases of goods and materials, employment number and costs, inventories, and net capital expenditure. Core ARD data are available from the 1970s until 2006. Since 1999 the ARD also contains a few questions of direct relevance for this research, such as expenditures on total energy used in the running of the business.

Detailed information on energy use is taken from the Quarterly Fuels Inquiry (QFI), a quarterly survey among a panel of about 1,000 manufacturing plants managed by the ONS on behalf of DTI. The survey collects data on prices and quantities for all relevant fuel types, including medium fuel oil, heavy fuel oil, gas oil, liquefied petroleum gas (LPG), coal (graded, smalls), hard coke, gas (firm contract, interruptible contract, tariff), and electricity. We have data for the period from 1993 to 2004. The majority (83%) of the observations in the QFI can be matched to the ARD without difficulty because both surveys use the same underlying government business register IDBR as their sampling frame. However, due to random sampling in the ARD we do not have ARD data for all QFI plants.[20]

We gathered information on CCA participation from both the DEFRA and HM Revenue and Customs (HMRC) websites. Lists of facilities in the original sector agreements were downloaded from DEFRA's website. The agreements stipulate the certification periods and the sector targets along with the details on the calculation of the units of energy used and carbon emissions. They also contain a list of all facilities initially covered by the CCAs. Seven agreements lack sufficient information on the facilities covered by the CCA and thus had to be excluded from the analysis.[21] The HMRC website provides, sector by sector, the list of facilities that have joined the CCA along with the date of publication.[22] The lists are regularly updated and facilities that have resigned from the CCA are removed. We merged the DEFRA and HMRC lists to obtain a complete list of facilities that pay the reduced rate of the CCL. We match this information to the ARD and QFI by combining information on a plant's postcode and the UK

[20]For more details on the QFI and its combination with ARD data see Martin (2006).

[21]The craft baking sector and the meat processing sector do not contain a list of facilities. Another five sectors lack facility addresses, namely the NFU poultry meat production sector, the pig farming sector, the egg production sector, the British Poultry Meat Federation farms sector, and the British poultry meat federation processing sector.

[22]The date of publication is the date from which the CCA is applicable.

Company Register Number (CRN).

To construct the instrumental variable, we downloaded publicly available data from the European Pollution Emissions Register (EPER) which covers all European facilities regulated under the IPPC directive whose emissions exceed the reporting thresholds. The 2001 EPER file contains reporting thresholds and pollution discharges into air and water for 50 pollutants and covers 2,397 facilities in 56 sectors of activity in the UK. We construct the instrumental variable *NEPER* as a dummy variable that equals one if a facility is not on the EPER list, i.e. it does not report emissions of any of the pollutants regulated under PPC legislation. A value of zero is assigned otherwise. Just like the treatment variable T, this variable is zero for all plants before 2001 and does not vary between 2001 and 2004. To match EPER facilities to plants in our dataset we use the same algorithm that we used for matching CCA participation data.

4.2 Descriptive statistics

Table 2 summarizes the main variables from the ARD and QFI datasets for our regression sample.[23] ARD variables include age, number of employees, gross output, variable cost, capital stock, materials, energy expenditure (including CCL payments), as well as the percentage of energy expenditures in gross output and in variable costs (the sum of expenditures on materials, energy and wages). There is a substantial amount of dispersion between plants in energy intensity. For example, the energy expenditure share in gross output of a plant at the 90th percentile is more than 12 times larger than that of a plant at the 10th percentile. The QFI variables are electricity, liquid fuels (including oil, petrol, and LPG), gas, solid fuels such as coal, and total energy use. We report both quantities consumed and expenditures paid for all fuel variables. Moreover, we compute the share of gas in the consumption of both gas and electricity, as well as in total kWh consumed. We also compute total CO_2 emissions (in thousands of tonnes) on the basis of the fuel mix.

The regression sample starts in 1999 – the first year for which energy data are available in the ARD – and covers the first two target periods, i.e. from April 2001 until December 2004.

[23]To limit the effect of outliers we dropped 1,535 plants for which growth in the outcome variables were in the top and bottom percentiles.

Table 2: Descriptive statistics - ARD and QFI samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. ARD variables	**Mean**	**SD**	**SD, between**	**SD, within**	**p10**	**p90**	**Observations**
Age	19	10	10	1	6	31	16,917
Employment (L)	323	651	570	107	26	697	16,917
Gross output (GO)	49.027	188.895	161.035	29.411	1.584	87.423	16,917
Variable costs (Vcost)	42.588	172.272	139.901	26.675	1.370	76.626	16,917
Capital stock (K)	28.310	114.681	91.544	7.210	0.821	51.980	16,855
Materials (M)	33.891	154.088	124.064	25.257	0.774	57.769	16,917
Energy expenditures (EE)	0.745	3.681	3.735	0.467	0.020	1.324	16,917
Energy share in GO (EE/GO)	0.019	0.022	0.024	0.005	0.005	0.035	16,917
Energy share in Vcost (EE/VCost)	0.021	0.024	0.021	0.005	0.005	0.040	16,917
B. QFI variables							
Electricity (El)	17,011.950	66,416.140	86,953.840	8,341.068	505.111	33,211.610	5,309
Electricity expenditures (ElE)	528.504	1,528.922	1,797.534	233.577	25.895	1,148.033	5,309
Liquid fuels (Li)	3.630	91.200	78.171	62.430	0.000	0.050	5,309
Liquid fuels expenditures (LiE)	174.368	4,052.711	3,595.789	2,713.670	0.000	6.732	5,309
Gas (Gas)	31,833.620	120,328.800	128,243.700	34,859.000	0.000	62,193.680	5,309
Gas expenditures (GasE)	203.032	722.479	790.660	253.727	0.000	413.315	5,309
Gas share in Gas+El (Gas/(Gas+El))	0.221	0.186	0.171	0.077	0.000	0.480	5,309
Gas expenditure share (GasE/(GasE+ElE))	0.482	0.300	0.290	0.106	0.000	0.828	5,309
Gas share (Gas/kWh)	0.450	0.298	0.289	0.104	0.000	0.812	5,309
Solid fuels (So)	1.565	12.448	13.539	5.061	0.024	2.074	1,960
Solid fuels expenditures (SoE)	179.891	1,186.194	1,333.234	469.620	4.616	294.340	1,960
Solid fuels share (So/kWh)	0.068	0.135	0.138	0.039	0.000	0.311	5,309
Total kWh (kWh)	83,389.230	838,907.000	794,883.000	494,239.800	1,383.513	109,643.900	5,309
Total kWh expenditures (kWhE)	968.742	5,748.820	5,981.250	2,842.255	38.142	1,688.767	5,309
Total kWh over GO (kWh/GO)	1,323.874	2,468.517	2,507.119	771.367	127.655	3,068.232	4,097
CO2 (CO2)	30,450.940	281,918.300	271,437.400	163,734.700	621.472	41,447.790	5,309
CO2 intensity of energy use (CO2/kWh)	0.438	0.126	0.121	0.050	0.299	0.636	5,309
CO2 over GO (CO2/GO)	489.735	802.448	866.547	245.052	60.160	1,137.594	4,097

Notes: Descriptive statistics for the ARD pooled sample (1999-2004) and descriptive statistics for the QFI pooled sample (1997-2004). The variables GO, K, VCost, M and all the expenditure variables are in thousands of pounds. Total kWh, Gas and El are in thousands of kWh. So and Li are in thousands of tonnes. The CO2 variable measures total CO2 emissions in thousands of tonnes based on fuel use (the conversion factors are obtained from the Entech Utility Service Bureau, for more details see Martin, 2006).

Table 3: Descriptive statistics in 2000 by CCA participation status

	(1) CCL=0	(2) CCL=1	(3) Diff.	(4) NEPER=0	(5) NEPER=1	(6) Diff.
A. ARD variables						
Energy share in gross output	-3.881	-4.385	***	-3.739	-4.340	***
ln(EE/GO)	697	3,851		243	4,305	
Energy share in var. costs	-3.724	-4.251	***	-3.584	-4.204	***
ln(EE/VCost)	697	3,851		243	4,305	
Energy expenditure	6.458	4.662	***	7.205	4.810	***
ln(EE)	697	3,851		243	4,305	
Real gross output	10.340	9.048	***	10.943	9.150	***
ln(Real GO)	697	3,851		243	4,305	
Employment	5.659	4.726	***	5.872	4.812	***
ln(L)	697	3,851		243	4,305	
Capital stock	9.946	8.402	***	10.536	8.532	***
ln(K)	697	3,830		243	4,284	
Materials	9.774	8.437	***	10.435	8.541	***
ln(M)	697	3,851		243	4,305	
Age	20.122	17.676	***	19.226	17.984	-
	697	3,851		243	4,305	
B. QFI variables						
Electricity	16.311	15.068	***	17.516	15.193	***
ln(El)	149	368		52	465	
Gas	16.867	15.268	***	17.926	15.516	***
ln(Gas)	123	301		38	386	
Gas share in gas + electricity	0.245	0.183	***	0.206	0.200	-
(Gas/(Gas+El))	149	368		52	465	
Gas share	0.482	0.413	*	0.376	0.439	-
(Gas/kWh)	149	368		52	465	
Solid fuels	5.827	5.224	*	6.416	5.212	***
ln(So)	60	138		32	166	
Solid fuels share	0.046	0.083	**	0.066	0.073	-
(So/kWh)	149	368		52	465	
Total kWh	17.487	16.085	***	18.614	16.252	***
ln(kWh)	149	368		52	465	
CO2	16.599	15.251	***	17.787	15.400	***
ln(CO2)	149	368		52	465	

Notes: Summary statistics for the year 2000 by CCL and NEPER status. For each variable, we report the mean and the number of observations in the row below the variable mean. We report the natural logarithm for all variables except age. Columns 3 and 6 report significance levels of a t-test of differences in group means with unequal variance, at ≤1% (***), ≤5% (**), ≤10% (*).

This window of analysis avoids possible complications due to (i) an overlap with the EU ETS which affected about 500 CCA plants from 2005 onwards, (ii) adjustments of CCA targets for the third milestone period, and (iii) new entry of sectors in 2006 following changes in the eligibility rules.

Table 3 displays descriptive statistics from both samples in the pre-treatment year 2000, broken down by treatment status. The treatment variable *CCL* takes a value of one if a plant pays the full tax rate and a value of zero if the plant participates in a CCA. It also reports the results of a *t*-test of equality of the group means (assuming unequal variance of the two groups). It is evident that participation in CCAs is not random: CCA plants are, on average, older, larger and more energy intensive, and for most of these plant characteristics equality between CCL and CCA plants is rejected at the 1% significance level. In view of the strong correlation between treatment status and observable plant characteristics, we cannot rule out that unobservable plant characteristics also influence selection. The difference equation (2) controls for this type of bias provided that the outcome variable follows a common trend across treatment and control groups prior to treatment. We thus plot the trends in the outcome variables over the sample period and calculate pre-treatment growth rates by both treatment and eligibility status (cf. Figure C.1 and Table C.2 in the Appendix, respectively). Based on this, we cannot reject the common trends assumption.

5 Results

5.1 Determinants of CCL status

For *NEPER* to be a valid instrument, it must be sufficiently correlated with CCL status conditional on other controls. Table 4 reports the results from various regressions of CCL status on *NEPER* and other plant characteristics. Each regression is run in both the ARD and the QFI sample. The specification underlying the results in columns 1 and 5 is a simple linear regression of *CCL* on *NEPER* in the cross section for the year 2001. The results show that the instrumental variable *NEPER* is a strong predictor of CCL status. Columns 2 and 6 report the marginal effects from a probit regression of the same specification. The coefficients imply that a value of *NEPER=1* increases a plant's chances of paying the tax in full by 28.3% in the ARD sample and by 44% in the QFI sample. Results from the actual first-stage equation (3) are reported in columns 3 and 7 and show that there is a robust positive and statistically significant relationship between the treatment variable and the instrument. Columns 4 and 8 display the results from a probit regression of CCL status in 2001 on various plant level controls evaluated at their 2000 levels. The coefficient estimates show that the simple correlations between CCL status and plant characteristics we found in Table 3 persist even when we control for sectoral differences. In particular, plants that were larger in terms of their capital, labour and energy inputs prior to treatment were more likely to participate in a CCA. Interestingly, we obtain a positive coefficient on gross output. A plausible explanation for this is that, conditional on size, plants that expanded their output in the year before the CCL package was introduced were less inclined to participate in a CCA as an expansion would make it more difficult to meet their CCA target.

5.2 Average treatment effects on CCL plants

Table 5 reports regression results for various outcome variables from the ARD (panel A) and the QFI (panel B). Column 1 reports OLS estimates of the treatment coefficient α in equation (2) and column 2 reports the OLS estimate of the coefficient $\tilde{\alpha}$ in the reduced-form equation (5). Column 3 reports the average treatment effect on CCL plants as identified by the IV regression equation (4).

The first two rows in panel A of Table 5 report the results for energy intensity measured as energy expenditures over gross output and as the share of energy expenditures in variable costs, respectively. We find that the CCL caused plants to decrease their energy intensity relative to CCA plants. The point estimates from the IV regressions are -0.172 for the former measure and -0.202 for the latter. The effects are both economically and statistically significant. The importance of controlling for selection is evident from the sizable differences between the OLS and IV estimates. In particular, OLS estimation leads to an upward bias when estimating the effect of the CCL on the growth in energy intensity. The direction of the bias is consistent with plants choosing to participate in a CCA if they anticipated a negative shock to their energy

Table 4: Determinants of CCL status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable				**CCL status**				
Sample		**ARD sample**				**QFI sample**		
Time period	**2001**	**2001**	**2000-2004**	**2001**	**2001**	**2001**	**1998-2004**	**2001**
Method	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**	**Probit**
NEPER	0.320***	0.283***	0.348***		0.253***	0.440***	0.350***	
	(0.036)	(0.039)	(0.035)		(0.070)	(0.090)	(0.060)	
lnGO(t-1)				0.032***				0.033
				(0.012)				(0.082)
lnK(t-1)				-0.037***				-0.231***
				(0.010)				(0.069)
lnEE(t-1)				-0.043***				-0.105**
				(0.007)				(0.044)
lnL(t-1)				0.022***				0.147**
				(0.010)				(0.065)
Sector controls	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	0.296	0.296	0.652	0.382	0.421	0.280	0.722	0.359
Observations	4,330	4,037	16,917	3,985	569	436	6236	424

Notes: Probit results report the marginal effect on the probability of being subject to the full-rate CCL. All regressions additionally include age, age squared, and regional trends. Standard errors in parenthesis are robust to heteroskedasticity and autocorrelation (except probit models). Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Table 5: Impact of the CCL on plant outcomes

	(1)	(2)	(3)	(4)
Dependent variables	**OLS**	**RF**	**IV**	**Obs./ Plants**
A. ARD variables				
Energy share in gross output	-0.022*	-0.054**	-0.172**	16,917
Δln(EE/GO)	(0.013)	(0.022)	(0.071)	6,901
Energy share in var. costs	-0.025*	-0.064***	-0.202***	16,917
Δln(EE/VCost)	(0.013)	(0.022)	(0.071)	6,901
Energy expenditure	-0.019	-0.027	-0.085	16,917
Δln(EE)	(0.013)	(0.019)	(0.062)	6,901
Real gross output	0.004	0.027	0.087	16,917
Δln(Real GO)	(0.011)	(0.017)	(0.054)	6,901
Employment	0.009	0.025	0.078	16,917
Δln(L)	(0.011)	(0.017)	(0.054)	6,901
Total factor productivity	0.001	0.000	0.001	16,851
Δln(GO)~inputs	(0.006)	(0.011)	(0.033)	6,866
B. QFI variables				
Electricity	-0.033	-0.069**	-0.226**	4,587
Δln(El)	(0.022)	(0.031)	(0.109)	1,079
Gas	-0.053	0.052	0.165	3,748
Δln(Gas)	(0.037)	(0.044)	(0.156)	908
Gas share in gas + electricity	-0.024***	0.035*	0.114	4,587
Δ(Gas/(Gas+El))	(0.009)	(0.020)	(0.073)	1,079
Gas share	-0.023**	0.020	0.060	4,602
Δ(Gas/kWh)	-0.011	-0.016	-0.054	1,082
Solid fuels	0.174*	0.101	0.460	1,563
Δln(So)	(0.096)	(0.156)	(0.654)	445
Solid fuels share	0.000	0.010	0.020	4,605
Δ(So/kWh)	-0.004	-0.008	-0.025	1,082
Total kWh	-0.106***	-0.008	-0.027	4,605
Δln(kWh)	(0.027)	(0.037)	(0.114)	1,082
CO2	-0.074***	-0.030	-0.096	4,605
Δln(CO2)	(0.022)	(0.030)	(0.093)	1,082

Notes: Column 1 displays the OLS coefficient on the treatment variable, column 2 displays the OLS coefficient on the instrumental variable in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable. Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. All regressions include age, age squared, as well as dummies for year, region and 3-digit industry code. In panel A, the total factor productivity regressions also control for labor, capital stock, and for expenditures on materials and energy. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

intensity growth, as this made it easier to comply with the CCA target. This would create a positive correlation between the disturbance term and the treatment variable in equation (2), and it is also consistent with the results from the probit regression above which showed a positive association between CCA participation and a higher output in 2001 conditional on energy and other inputs in 2000.

In rows 3 and 4, we break down the effect on energy intensity by looking at its components. The IV point estimates of -0.085 for energy expenditure and 0.087 for real gross output suggest that CCL plants both reduced energy and increased gross output so as to achieve the reductions in energy intensity reported in row 1. However, these effects lack statistical significance at conventional levels.[24] Similarly, we obtain a positive but not statistically significant point estimate for employment of 0.078.

We derive an estimate of the CCL impact on TFP from an augmented equation (4) which includes the production factors capital, labor, materials, and energy. This amounts to estimating a production function where the treatment variable captures the impact of the CCL on otherwise unexplained differences in TFP.[25] The coefficients reported in row 6 are positive but small in magnitude and lack statistical significance. We thus cannot reject the hypothesis that the CCL had no effect on plant-level TFP.

The evidence in panel A clearly shows that the CCL led to substantial improvements of plant-level energy efficiency compared to the CCA. As the CCL was not part of a harmonized carbon tax across Europe, this begs the question of whether the CCL jeopardized the competitiveness of UK industry. If this was the case, we should find negative tax coefficients for employment and output because plants that pay the full rate of the levy scale down production and employment relative to the control group. However, our results do not support such concerns as the point estimates are positive and lack statistical significance.

It is less clear from the results in panel A whether the improvement in energy intensity was brought about by technological change or by movements along the production isoquant. While

[24]The lack of statistical significance of the coefficient on energy expenditures could be the result of a negative effect on energy demand which is partially offset by the increase in the after-tax price of energy.

[25]This controls for production function endogeneity arising from plant specific unobserved effects (Griliches and Mairesse, 1995).

the point estimates are consistent with an increase in the scale and the substitution of labor for energy, we cannot reject the hypothesis that the scale of operations remained unchanged at the treated plants. From a climate-policy perspective, it is important to know, however, whether reductions in energy expenditures in CCL plants actually occurred, whether they correspond to reductions in energy consumption and whether they lowered carbon emissions. For example, instead of consuming less of all fuel types CCL plants might substitute towards fuels that are cheaper but also more polluting, such as coal. More detailed information on energy use is needed to address this issue, as the energy expenditures variable lumps together changes in the *tax-inclusive* price and quantity of energy, as well as the effects of substitution between different fuel types.

Panel B of Table 5 reports results from regressions using *quantity* changes in energy consumption by fuel type which are available in the QFI sample. Although this sample is smaller than the ARD sample, we find economically and statistically significant evidence that the CCL caused plants to decrease their electricity use by 22.6%. The strong response underlines the fact that the CCL imposes the highest tax rate for electricity. For both gas and solid fuels we obtain positive point estimates of the treatment effect, both in absolute terms and as a share of total kWh consumed.[26] While these coefficients are not estimated with enough precision to be conclusive, they hint at the possibility that CCL plants switched from electricity to the lower taxed fuels gas and coal. This would also explain why the statistical significance of the IV coefficient for electricity does not carry over to the one for total kWh in row 10. If plants switch from electricity to gas or coal they are likely to require more kWh of primary energy to achieve a given level of energy services. This might account for at least a partial offset of a tax-induced reduction in the demand for those services.

The significant decrease in electricity consumption among CCL plants translates into a decrease in carbon dioxide emissions *ceteris paribus*, but this could be offset by an increase in the consumption of other fuel types. The last row of Table 5 shows the impact of the CCL on total CO_2 emissions, calculated as the sum of emissions across fuel types. The CCL is

[26]We report gas use as a share of total kWh and as a share of gas and electricity only, as other fuels are less frequently used. The regressions on solid fuels are conditional on a plant using solid fuels in at least one period. In contrast, the solid fuels share is computed for all plants and takes the value of zero for plants that do not use it.

associated with a significant decrease in total CO_2 emissions of 7.4% in the OLS regression. The point estimate increases slightly when going from OLS to IV, yet statistical significance is lost. We conjecture that this is due to the noisy estimates of the tax response for fuels other than electricity. In the absence of a larger sample that would enable us to estimate this effect with more precision, there are two possible ways of quantifying the effect of the CCL on carbon emissions. On the one hand, one can choose to disregard statistically insignificant coefficients altogether and conclude that the unchecked decrease in electricity consumption translates into a decrease in CO_2 emissions of equal magnitude. On the other hand, a more cautious interpretation of the results is to use the point estimate of -0.096 from the IV estimation which accounts for the possibility that some CCL plants switched into dirtier fuels such as coal. We thus conclude that the CCL – though not designed as a pure carbon tax – caused plants paying the full rate to reduce CO_2 emissions by between 9.6% and 22.6% compared to plants that paid the reduced rate.

5.3 The treatment effect over time

The time profile of the treatment effect is of interest because it can reveal a possible time delay in plants' responses to the treatment, or whether the treatment effect dies off after a while. We estimate the time profile by interacting the CCL variable with dummy variables for post-treatment years 2001-2004 and substituting them for the simple treatment dummy in the regression equation.

Table 6 displays the annual treatment coefficients for the ARD variables. For energy intensity the negative CCL impact is present from 2001 onwards. The differences in point estimates for different years are well within the margins of sampling error. The coefficients on energy expenditures, real gross output and employment have the same signs as in Table 5. They are most precisely estimated for 2001, the first year of treatment. The point estimates in later years always have the same sign but lack statistical significance. Again, there is no statistically significant effect of the CCL on TFP.

Table 7 displays the time profile of treatment effects in the energy quantity regressions based on QFI data. The effect on electricity consumption is always negative but becomes sig-

Table 6: CCL impact by year - ARD outcome variables

Dependent variables	Year	(1) OLS	(2) RF	(3) IV	(4) Obs./ Plants
Energy share in gross output	2001	-0.024*	-0.071***	-0.189***	16,917
		(0.014)	(0.021)	(0.057)	6,901
Δln(EE/GO)	2002	-0.012	-0.048*	-0.160*	
		(0.017)	(0.028)	(0.085)	
	2003	-0.011	-0.035	-0.139	
		(0.021)	(0.034)	(0.111)	
	2004	-0.047**	-0.054	-0.202	
		(0.024)	(0.040)	(0.145)	
Energy share in var. costs	2001	-0.018	-0.071***	-0.191***	16,917
		(0.014)	(0.020)	(0.057)	6,901
Δln(EE/VCost)	2002	-0.020	-0.062**	-0.201**	
		(0.016)	(0.028)	(0.086)	
	2003	-0.022	-0.050	-0.189*	
		(0.019)	(0.034)	(0.111)	
	2004	-0.048**	-0.068*	-0.258*	
		(0.023)	(0.039)	(0.142)	
Energy expenditure	2001	-0.021	-0.035*	-0.088*	16,917
		(0.014)	(0.019)	(0.051)	6,901
Δln(EE)	2002	-0.007	-0.005	-0.038	
		(0.017)	(0.026)	(0.077)	
	2003	-0.012	-0.014	-0.072	
		(0.019)	(0.029)	(0.094)	
	2004	-0.038*	-0.057	-0.194	
		(0.023)	(0.037)	(0.133)	
Real gross output	2001	0.002	0.036**	0.101**	16,917
		(0.009)	(0.015)	(0.041)	6,901
Δln(Real GO)	2002	0.005	0.043**	0.122*	
		(0.013)	(0.021)	(0.063)	
	2003	-0.001	0.021	0.067	
		(0.017)	(0.025)	(0.083)	
	2004	0.009	-0.003	0.008	
		(0.021)	(0.034)	(0.121)	
Employment	2001	0.012	0.026*	0.073*	16,917
		(0.013)	(0.015)	(0.039)	6,901
Δln(L)	2002	0.002	0.030	0.090	
		(0.013)	(0.019)	(0.057)	
	2003	0.002	0.039	0.112	
		(0.016)	(0.033)	(0.099)	
	2004	0.024	-0.003	0.014	
		(0.020)	(0.031)	(0.113)	
Total factor productivity	2001	0.002	0.010	0.028	16,851
		(0.007)	(0.009)	(0.025)	6,866
Δln(GO)	2002	0.001	0.005	0.011	
		(0.008)	(0.013)	(0.040)	
	2003	-0.003	-0.010	-0.033	
		(0.010)	(0.015)	(0.049)	
	2004	0.005	-0.013	-0.049	
		(0.011)	(0.018)	(0.068)	

Notes: Column 1 displays the OLS coefficient on the treatment variable interacted with dummies for post-treatment years. Column 2 displays the OLS coefficient on the instrumental variable (and year interactions) in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable (and year interactions). Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. All regressions include age, age squared, and control for year, region and 3-digit industry effects. The total factor productivity regressions also control for labor, capital stock, and for expenditures on materials and energy. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Table 7: CCL impact by year - QFI outcome variables

		(1)	(2)	(3)	(4)
Dependent variables	**Year**	**OLS**	**RF**	**IV**	**Obs./ Plants**
	2001	-0.022	-0.012	-0.039	4,587
Electricity		(0.019)	(0.033)	(0.096)	1,079
	2002	-0.034	-0.096***	-0.320**	
Δln(El)		(0.025)	(0.036)	(0.137)	
	2003	-0.037	-0.119***	-0.407**	
		(0.035)	(0.046)	(0.186)	
	2004	-0.051	-0.093	-0.386*	
		(0.046)	(0.058)	(0.230)	
	2001	0.009	0.111**	0.308**	3,748
Gas		(0.036)	(0.050)	(0.155)	908
	2002	-0.092**	-0.004	0.006	
Δln(Gas)		(0.045)	(0.063)	(0.186)	
	2003	-0.088	0.008	0.051	
		(0.057)	(0.080)	(0.270)	
	2004	-0.098	0.058	0.204	
		(0.076)	(0.104)	(0.425)	
	2001	-0.021**	0.028	0.083	4,587
Gas share		(0.008)	(0.020)	(0.063)	1,079
	2002	-0.033***	0.042*	0.138	
Δ(Gas/kWh)		(0.011)	(0.024)	(0.086)	
	2003	-0.024*	0.036	0.127	
		(0.013)	(0.023)	(0.090)	
	2004	-0.014	0.038	0.150	
		(0.016)	(0.028)	(0.114)	
	2001	-0.007	-0.007	-0.019	4,605
Solid fuels share		(0.004)	(0.009)	(0.026)	1,082
	2002	-0.001	0.017	0.052	
Δ(So/kWh)		(0.005)	(0.015)	(0.047)	
	2003	-0.003	0.003	0.021	
		(0.006)	(0.011)	(0.036)	
	2004	0.002	0.026*	0.089*	
		(0.007)	(0.014)	(0.052)	
	2001	-0.080***	0.029	0.080	4,605
Total kWh		(0.025)	(0.042)	(0.121)	1,082
	2002	-0.140***	-0.008	-0.040	
Δln(kWh)		(0.034)	(0.057)	(0.168)	
	2003	-0.123***	-0.109*	-0.317*	
		(0.044)	(0.056)	(0.182)	
	2004	-0.084	0.040	0.062	
		(0.054)	(0.070)	(0.239)	
	2001	-0.055***	0.019	0.051	4,605
CO2 emissions		(0.020)	(0.036)	(0.102)	1,082
	2002	-0.094***	-0.037	-0.135	
Δln(CO2)		(0.026)	(0.040)	(0.118)	
	2003	-0.081**	-0.122**	-0.378**	
		(0.036)	(0.048)	(0.171)	
	2004	-0.073	-0.011	-0.115	
		(0.045)	(0.057)	(0.195)	

Notes: Column 1 displays the OLS coefficient on the treatment variable interacted with dummies for post-treatment years. Column 2 displays the OLS coefficient on the instrumental variable (and year interactions) in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable (and year interactions). Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. All regressions include age, age squared, and control for year, region and 3-digit industry effects. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

nificant only after 2001. Conversely, the impact on gas consumption is positive and statistically significant in 2001 only. This suggests that CCL plants initially switched to gas but, from 2002 onwards, managed to reduce electricity consumption without significantly increasing consumption of other fuels, thus reducing the number of total kWh consumed as well as CO_2 emissions.

5.4 Robustness checks

5.4.1 Balanced sample

Our sample is an unbalanced panel for a number of reasons: random sampling of smaller plants in the ARD; plant births and deaths; a few plants missing responses in some years. As the set of plants in the sample changes slightly from year to year, the time profile of the treatment effect might reflect – at least in part – the changes in sample composition rather than the dynamic response to the CCL. Another potential problem with the unbalanced panel is that the results could be dominated by potentially more extreme responses of exitors. We therefore re-estimate the model with time interactions in a subset of "stayer" plants with observations in all years after 1999. The results are displayed in Tables C.3 and C.4 in the Appendix. The sample size drops by about half in both samples, and the 2001 coefficients become statistically insignificant except for gas. This may be due to the smaller sample size but also to the fact that plants were subject to treatment only during the last three quarters of that year as CCL was introduced on April 1st. Apart from that, the qualitative findings obtained in the full sample are confirmed in the sample of stayers. The point estimates on the ARD energy variables are negative as in the full sample, and even larger in magnitude towards the end of the sample period. There are no statistically significant effects on output, employment and TFP. This suggests that energy efficiency improvements at treated plants were achieved by reducing energy expenditures rather than increasing output. The results for the QFI sample are both qualitatively and quantitatively close to those obtained in the unbalanced sample, although they are generally estimated with less precision. In sum, the impacts of the tax estimated using the unbalanced panel do not seem to be a result of changes in the sample composition.

5.4.2 Controlling for unobserved trends

Our identification strategy relies on the assumption that there is no unobserved heterogeneity in the difference equation (2). As shown in Section 4.2, the raw data support this "common trends" assumption. In regressions summarized in Tables C.5 and C.6, we include a time-invariant eligibility dummy in equation (2) to allow for the possibility that unobserved trends in the outcome variables differ between eligible and non-eligible plants. This yields qualitatively similar results, albeit less statistically significant ones in later years. Since the coefficients on the additional control are statistically insignificant for all outcome variables except solid fuels, we do not include them in our preferred specification.

5.4.3 Common support regression

Despite our IV strategy there might be concern that results are driven by a fundamental heterogeneity between treated (eligible) and non-treated (non-eligible) plants . Therefore, as a robustness test we restrict the control group to a common support which is identified by the predicted probability of a plant in the control group to receive treatment.[27] We construct this common support sample by dropping plants that do not belong to the central 80% of the propensity score distribution so as to balance the covariates between the treatment and the control group.[28] We then re-estimate the main specification using only observations that belong to the common support sample. The results are reported in Table C.7. For the ARD variables in panel A this leads to slightly larger point estimates, suggesting that heterogeneity within the treated group is not a major problem. In the smaller QFI dataset, about half of the sample needs to be dropped, but this entails no qualitative change to the results.

[27]See Blundell et al., (2004) for a framework that combines propensity score matching with a differences-in-differences estimator.

[28]Propensity scores are computed as the predicted values of a probit regression of CCL status on plant characteristics (as in columns 4 and 8 of Table 4) for the year 2000. We drop the top and bottom 5% of the resulting distribution. We iterate on this procedure once and verify that covariates in the resulting sample are balanced.

6 Heterogeneous impacts, aggregate effects, and plant exit

6.1 The impact of the CCL in different subsamples

Our discussion so far has focused on the average effect of the CCL on treated plants. Apart from this, it is useful to know how the impact of the CCL varies across treated plants with certain characteristics. For example, the tax impact may differ from the ATT in industries that are very energy intensive because the levy imposes a higher cost burden on these industries. Moreover, as the political cost of job losses is high, policy-makers might be interested in the tax impact on small firms which are responsible for the bulk of total employment. Finally, the impact of the CCL on competitiveness may be particularly high for firms in sectors with high import penetration, as foreign competition prevents them from passing compliance cost on to their customers through higher output prices.

To shed light on this, we estimate the impact of the CCL separately for plants with more vs. less than 250 employees, and for plants with high vs. low energy or trade intensities in 2000 or 1999. The splitting points for energy and trade intensities are defined at the 3-digit and 4-digit sector level, respectively, in the following fashion. First, average intensities are computed across plants in each sector. Next, sectors are sorted in the order of increasing intensity. Starting with the sector with the highest intensity, we then assign sectors to the high intensity group until approximately 50% of plants are assigned to this group. The remaining plants are assigned to the low intensity group.

The first two columns of Table 8 report the IV coefficients for the split by energy intensity, defined as the share of energy expenditures in gross output. Results for the low and high intensity groups are reported in the odd and even-numbered columns, respectively. The IV point estimates for energy intensity and energy expenditures indicate that the average effects reported in Table 5 are due to a strong response by plants in the more energy intensive sectors. The point estimates in this group are -0.195 for energy intensity and -0.154 for energy expenditures in this group and both are statistically significant at 5%. In contrast, the point estimates for the low intensity group lack statistical significance. The point estimates for electricity consumption are similar in magnitude but lack statistical significance in the group of less energy intensive plants.

Table 8: CCL impact in different sub-samples (IV coefficients)

		(1)	(2)	(3)	(4)	(5)	(6)
		Energy intensity		**Trade intensity**		**Size**	
Dependent variables		*low*	*high*	*low*	*high*	*low*	*high*
Energy share in gross output		-0.122	-0.195**	-0.094	-0.196*	-0.166	-0.149*
$\Delta\ln(EE/GO)$		(0.159)	(0.081)	(0.099)	(0.100)	(0.181)	(0.088)
Energy expenditure		0.107	-0.154**	-0.059	-0.090	-0.238	0.029
$\Delta\ln(EE)$		(0.131)	(0.072)	(0.089)	(0.084)	(0.164)	(0.077)
Employment		0.194	0.047	0.092	0.060	-0.074	0.113
$\Delta\ln(L)$		(0.143)	(0.054)	(0.090)	(0.068)	(0.105)	(0.090)
Electricity		-0.247	-0.233*	-0.321	-0.110	-0.059	-0.286*
$\Delta\ln(El)$		(0.235)	(0.138)	(0.252)	(0.110)	(0.175)	(0.161)
ARD sample	obs.	8,081	8,836	8,137	7,871	10,170	6,718
	plants	3,291	3,610	3,216	3,213	4,914	1,977
QFI sample	obs	2,001	2,586	1,994	2,318	2,122	2,274
	plants	470	609	461	552	513	450

Notes: The table reports IV estimates of the CCL impact on various plant-level outcomes obtained in different sub-samples. Energy and trade intensity samples are split according to the median defined at the 3-digit and 4-digit sector level, respectively, in 1999 or 2000. Size is defined based on employment at the respondent unit, those with 250 employees or less in 2000 or 1999 qualified as small. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

In columns 3 and 4 of Table 8 we split the sample according to the trade intensity in 4-digit NACE sectors, which is computed as the value of imports and exports to non-EU countries over the total market size within the EU27.[29] This measure has been used by the EU Commission to gauge the competitiveness impact of the EU ETS on manufacturing firms. To the extent that trade intensity measures the degree of competition from non-regulated countries, it picks up the (lack of) ability of firms to pass on the cost of the CCL to their customers. The point estimates for the ARD variables in the high trade intensity group closely follow those obtained in the full ARD sample. In contrast, the impact on energy intensity is not statistically significant in the low intensity group. It appears that the impact of the CCL on electricity consumption is more negative for the low intensity group than for the high intensity group – however, neither coefficient is estimated with precision. We do not find any significant impact on employment in either of the two groups. This gives rise to two interpretations: first, that trade intensity might not be a good criterion for identifying adverse effects on competitiveness; or second that the hypothesis which states that there are no such effects should not be rejected.

[29] Data on trade intensity were taken from the Impact Assessment accompanying the "Commission Decision determining a list of sectors and subsectors which are deemed to be exposed to a significant risk of carbon leakage pursuant to Article 10a (13) of Directive 2003/87/EC", of September 4, 2009.

The last two columns of Table 8 report the results for the employment split. While the point estimates for energy intensity are close to the corresponding estimate for the full sample in both groups, only the coefficient for the larger plants is statistically significant at 10%. Similarly, the point estimate for electricity use is negative in both samples but significant only in the sample of larger plants. This suggests that the negative impacts of the CCL on energy intensity and electricity use are driven by the tax response of larger plants.

6.2 Aggregate effects of a carbon tax

While the micro-level approach allows for better identification of the causal impacts of the tax, from a policy point-of-view the aggregate implications of the tax matter. In this section, we calculate the aggregate impact of the CCL on energy consumption. Furthermore, we compute the effect of a counterfactual carbon tax similar to the CCL but without the reduced tax rate. This exercise allows us to compare our results to studies assessing the impact of energy price changes on fuel consumption at the aggregate level.

In order to estimate the impact of the CCL on the aggregate of an outcome variable Y, we weight the corresponding micro-level ATT estimate with the share in $\sum_i Y_i$ that treated plants (those paying the full tax rate) accounted for prior to treatment. This yields

$$\Gamma_Y = \left(e^{\hat{\alpha}} - 1\right) \frac{\sum_{\{i|T_i=1\}} Y_{i,2000}}{\sum_i Y_{i,2000}}.$$

For energy expenditure, the share of fully taxed plants is given by 52.42% and hence $\Gamma_{EE} = -4.27\%$. In the case of electricity, fully taxed plants accounted for 45.96% of total electricity consumption and hence $\Gamma_{El} = -9.30\%$.

When computing the aggregate impact of a counterfactual CCL without discounts, we need to take into account how plants that were not eligible for a discount would have behaved had they been eligible. Some of those plants would not have applied for a discount. For these ‘not tax-concerned types’ we make the conservative assumption that they do not respond to the tax at all. For all other plants (the ‘tax-concerned types’) we assume that their tax response equals the ATT. Since the type is not observed for the majority of plants in our sample, we predict the probability p_i that plant i is of the 'tax-concerned' type using the Probit regression summarized in columns 4 and 8 of Table 4. We then compute the aggregate impact $\hat{\Lambda}_Y$ by weighting each plant's impact by this predicted probability $\hat{p}_i$, multiplied by its share in the aggregate prior to treatment, i.e.

$$\hat{\Lambda}_Y = (e^{\hat{\alpha}} - 1)\frac{\sum_i (\hat{p}_i Y_{i,2000})}{\sum_i Y_{i,2000}}. \tag{6}$$

This yields $\hat{\Lambda}_{EE} = [\exp(-0.085) - 1] \cdot 0.57 = -4.64\%$ for energy expenditure and $\hat{\Lambda}_{El} = [\exp(-0.226) - 1] \cdot 0.59 = -11.93\%$ for electricity. According to this back-of-the-envelope calculation, had the CCL been applied to all plants without rebates, it would have decreased aggregate energy expenditures in manufacturing by at least 4.6% and aggregate electricity consumption by at least 11.9%.

One might ask what these estimates imply for the price elasticity of energy demand. Given that, on average, CCL plants pay $\frac{1.15}{1.03} - 1 =$11.7% more for energy, the implicit price elasticity of energy *expenditures* can be computed as $\eta_{EE} = |\frac{-0.046}{0.117}| = 0.39$. This implies an upper bound on the price elasticity of energy *demand* equal to $\eta_E = |-0.39 - 1| = 1.39$.[30] The elasticity of electricity demand can be computed in a similar fashion. Given that the CCL raised the electricity price by $\frac{0.43}{4.25} = 10.1\%$ for the average manufacturing plant (cf. Table 1), the tax differential between CCL plants and non-CCL plants is approximately $\frac{0.8 \times 0.43}{4.25 + 0.2 \times 0.43} = 7.9\%$. Hence the elasticity of electricity demand is given by $|\frac{-0.119}{0.079}| = 1.51$, which is slightly larger than the elasticity recovered in the ARD sample.

Both numbers are at the upper end of elasticity estimates obtained in comparable studies. For example, Bjorner and Jensen (2002) estimate the energy price elasticity at -1.37 in the pooled cross-section and -0.50 in a fixed-effects specification.[31] The reader should bear in mind, however, that we recover an estimate of a *tax-induced* price elasticity. Davis and Kilian (2010) argue that this is structurally different from elasticity estimates based on other kinds of price variation because taxes may be perceived as more persistent and hence induce larger behavioral changes. They also point to a possible additional effect of media coverage that

[30] If the CCL lowers the producer price (relative to CCA plants) then the elasticity of energy demand is less than 1.39. The extent to which energy suppliers can price-discriminate between customers who pay different tax rates depends largely on the type of fuel and on local market structure.

[31] Our OLS estimate in the difference equation implies an upper bound on the elasticity of 1.09 but – as we have argued above – this is biased towards zero if contracting firms select into CCAs.

accompanies the introduction of such taxes. Since the CCL was promoted as the UK's flagship regulation for mitigating climate change, there was ample scope for such an effect of the CCL, and our comparatively large estimates do not speak against this possibility.

Finally, notice that the IV point estimates are too large if we are underestimating the share of compliers $\Pr(CCL = 0|NEPER = 0)$. This possibility could arise because we were not able to match all CCA facilities when information on the business address or name was missing or wrong. In this case, the intent-to-treat (ITT) parameter or reduced-form coefficient reported in column 5 of Table 5 can provide a lower bound because it does not depend on the quality of the CCA match. The ITT point estimates for energy expenditures and electricity are -0.027 and -0.069, respectively. This translates into elasticity estimates of $|\frac{\exp -0.027-1}{0.117} - 1| = 1.23$ for energy demand and $|\frac{\exp -0.069-1}{0.079}| = 0.84$ for electricity demand which are both within the bounds derived using the simple approximation to the aggregate impact of the CCL.

6.3 The CCL and plant exit

The analysis so far has focused on how paying the full rate of the CCL affects various outcome variables in surviving plants. Rather than adjusting energy use and production at the intensive margin, there is a concern that firms might respond to the CCL by closing down plants altogether or by re-locating to non-regulated countries ("pollution havens"). After all, the substantial tax rebates granted under the CCA are intended to prevent such extensive-margin adjustments by energy intensive firms.[32]

We examine this by constructing a dummy variable $EXIT$ which equals 1 in the year of exit (defined as the year following the last reported year) and 0 otherwise. If exit occurs in year t, the plant is removed from the sample in subsequent years. We might be tempted to estimate the effect of the CCL on plant exit decisions by substituting $EXIT_{it}$ for the outcome variable in equation (4). However, since we do not observe any exit by treated plants in pre-treatment periods, all plants that exit prior to treatment are automatically assigned to the control group,

[32]Loss of international competitiveness and carbon leakage have been used with some success by industry to lobby against carbon taxes or similar regulations. Virtually all European governments that levy taxes on energy use or carbon emissions (i.e. Denmark, Finland, Germany, Netherlands, Sweden and the UK) have also granted exemptions or partial tax rebates to industries carrying a high tax burden.

causing the estimated treatment effect to be biased. To see this, recall that the differences-in-differences estimator of an exogenous treatment T is identified from the sample equivalent of the expression

$$\begin{aligned} \alpha \quad = \quad & E\left[Y_{it}\,|T_i=1,T_{it}=1\right]-E\left[Y_{it}\,|T_i=1,T_{it}=0\right] \\ & -E\left[Y_{it}\,|T_i=0,T_{it}=1\right]+E\left[Y_{it}\,|T_i=0,T_{it}=0\right] \end{aligned}$$

where T_{it} indicates the treatment period and $T_i=1$ indicates that a plant belongs to the treatment group. In the case of exit, by construction we have no exit in the treatment group, i.e. $E\left[EXIT_{it}\,|T_i=1,T_{it}=0\right]=0$. As a consequence, even in the case of an exogenous exit probability $\rho>0$ which is constant across plants and time periods (i.e. $\alpha=0$), this estimator is upwardly biased, since $\hat{a}=\rho-0-(\rho-\rho)=\rho>0$. IV estimation using *NEPER* is not a solution here because the instrument suffers from the same problem as the treatment variable (we falsely assign $NEPER=1$ to some plants that would have been listed in EPER had they survived until 2001).

To overcome this issue, we propose an IV estimator that exploits variation in pre-sample employment size. We define a dummy $SMALL_i$ which indicates that employment at the plant was below the median in 1997. Using data from 1998 onwards, we estimate the probit regression

$$\Pr\left(EXIT_{it}=1\right)=\Phi\left(\alpha CCL_{it}+SMALL_{i1997}+x_{it}'\beta\right). \tag{7}$$

This allows for fixed differences in the exit propensity between small and large plants and, since employment size and treatment status are strongly correlated (see Table 3), *SMALL* may also control to a large extent for fixed heterogeneity between treatment and control groups. Moreover, we use the interaction of $SMALL_i$ with a post-treatment dummy $\mathbf{I}_{\{t>2000\}}$ to instrument for CCL_{it}. The idea behind this is (i) to use the fact that size influenced the decision to participate in a CCA and (ii) to rely on variation in size prior to our sample period so as to preserve the exogeneity of the instrument. Unlike the ATT estimates reported above, the estimated coefficient α has the interpretation of a local average treatment effect (LATE).

Since all the information needed to estimate equation (7) is available from the IDBR, we

Table 9: Exit regressions

	(1)	(2)	(3)	(4)
	Probit	**RF**	**FS**	**IV**
CCL/SMALL * I{t>2000}	0.065***	0.000	0.025***	0.012
	(0.004)	(0.001)	(0.001)	(0.057)
SMALL	0.033***	0.034***	-0.001***	0.034***
	(0.001)	(0.001)	(0.000)	(0.001)
Observations	770,991	770,991	770,991	770,991

Notes: The table reports the results of probit (column 1) and IV probit (column 4) regressions of exit at the local unit level, along with reduced-form and first-stage regressions (columns 2 and 3, respectively). SMALL is a dummy indicating that employment at the plant was below the median in 1997. Coefficients in columns 1 and 4 are reported in terms of marginal effects w.r.t the probability of exit, evaluated at the mean of the explanatory variables. The sample period ranges from 1998 to 2004. All regressions include year dummies, age and age squared. Standard errors are clustered at the local unit level. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

implement these regressions at the local unit level (see footnote 19 above). Table 9 reports the results from probit and IV probit models, along with the corresponding reduced-form and first-stage results. In each of the exit regressions, the coefficient on *SMALL* is positive and significant; confirming the already well-documented empirical regularity that smaller firms are more likely to exit. The simple probit model yields a positive and significant coefficient estimate on *CCL* which implies a 6.5% increase of the exit probability at the average CCL plant. Notice that this effect is not necessarily causal. In fact, the positive coefficient is consistent with a reverse-causality explanation according to which, plants that anticipate to exit in the near future do not sign a CCA because the tax savings this generates over the remaining lifetime of the plant do not cover the fixed costs of certification to be paid upfront. Once we instrument for *CCL* status, the point estimate becomes statistically insignificant, as foreshadowed by the insignificant coefficient estimate on the instrument obtained in the reduced form. The first-stage regression coefficients show that our instrument is strongly correlated with CCL status. In sum, we find no evidence that the CCL had an impact on plant exit decisions. This finding is robust to the inclusion of industry controls and to splitting the sample by either energy or trade intensity as in Section 6.1 above.[33]

As a further robustness check, we estimate a version of equation (1) using employment data

[33]Table C.8 in the appendix reports reduced-form and first-stage results for the robustness checks. The coefficient estimates for the full sample with 2-digit sector dummies – reported in columns 1 and 2 – are virtually identical to the ones in Table 9. When the sample is split by energy or trade intensity – columns 2 through 5 – the coefficient estimates for the reduced form remain unchanged and the first-stage estimates change only in insignificant ways.

Table 10: CCL impact on employment at local units

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	**Energy intensity**		**Trade intensity**		**Size**	
		low	*high*	*low*	*high*	*small*	*large*
CCL	-0.012	-0.016	0.008	-0.013	0.015	-0.178	0.268
	(0.045)	(0.116)	(0.042)	(0.064)	(0.074)	(0.137)	(0.242)
NEPER	0.005	0.010	0.002	-0.003	0.020**	0.000	-0.036
*year diff	(0.005)	(0.008)	(0.005)	(0.007)	(0.008)	(0.007)	(0.022)
Observations	971,782	467,736	480,263	443,826	410,708	805,724	5,656
Plants	207,904	102,984	100,118	92,109	90,686	154,251	1,519

Notes: Columns display IV estimates of the impact of the CCL on log employment at the local unit level for different samples. The dependent variable is first-differenced from 1996 until 2000 and differenced at various intervals thereafter. NEPER is a dummy variable that equals one if a facility is not on the EPER list. Energy and trade intensity samples are split according to the median defined at the 3-digit and 4-digit sector level, respectively, in 1999 or 2000. Size is defined based on employment at the respondent unit, those with 250 employees or less in 2000 or 1999 qualified as small. All regressions include age, age squared, year dummies, a full set of region-by-year and 3-digit sector-by-year dummies. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

at the local unit level. Table 10 reports estimates of the CCL impact on employment in the full sample and when the sample is split according to energy and trade intensities, or size (defined as previously at the reporting unit). As before, we do not find evidence of a detrimental effect of the CCL on employment, regardless of which way the data are cut.[34]

7 Conclusion

There is widespread consensus that optimal climate policy should aim to regulate GHG emissions at minimal cost across a broad range of economic sectors. Although curbing industrial emissions must be an integral part of any such policy, there is surprisingly little empirical evidence on the impacts of large-scale regulations of industrial GHG emissions – let alone using market-based instruments. This paper evaluates the most salient such regulation implemented in the UK – the Climate Change Levy and negotiated agreements. Using a large panel of manufacturing plants from the UK production census allows us to circumvent two main weaknesses of previous evaluations. First, we avoid assumptions about macroeconomic or sectoral trends in energy use which need to be made in simulation studies to establish counterfactual ("baseline") emissions. Instead, we compare changes in plant outcomes both over time and between plants

[34]Our preferred estimates include a trend coefficient for the treatment group because we find it to be statistically significant for the high trade intensity group.

that were subject to different tax rates. The "baseline" is hence given by the contemporaneous outcomes of plants that faced lower tax rates by virtue of being in a CCA. Second, our estimates of the impact of the CCL are purged of confounding factors that affect plant performance at the level of the economy, the region and the sector. Since we also control for self-selection into CCAs by exploiting exogenous variation in CCA eligibility rules, we interpret our estimates as the causal effect of the CCL on plant outcomes.

We find robust evidence that the price incentive provided by the CCL led to larger reductions in energy intensity and electricity use than the energy efficiency or consumption targets agreed under the CCA. Tax rebates under the CCA were originally granted in order to shield energy intensive firms from competitiveness losses they might possibly suffer in international product markets as a consequence of the unilateral implementation of a significant energy tax. Contrary to this, we find no discernible impact on employment, output or productivity across groups, and we cannot reject the hypothesis that the CCL had no impact on plant exit. Our findings thus provide no justification for granting tax discounts. They do, however, make a strong case for the introduction of moderate energy taxes to encourage electricity conservation, to improve energy efficiency and to curb greenhouse gas emissions in the manufacturing sector. This is in contrast to previous research that attributed substantial carbon savings to the CCA scheme on the basis of comparisons with counterfactual baseline emissions (Ekins and Etheridge, 2006; Barker et al., 2007; AEAT, 2004).[35] While our research design arguably produces a more credible estimate of the effect of the CCL, it is clear that this effect is additional to any effect the CCA targets may have had on firm behavior.

This raises the question of whether alternative measures of putting a price on carbon emissions such as tradable permits would have yielded similar results. Since CCA firms were allowed to participate in the UK ETS, our results support the conclusion that the tax outperformed the cap-and-trade regulation. It seems very likely that this is the consequence of an overly generous cap. Neither previous research (Cambridge Econometrics, 2005; Ekins and Etheridge,

[35] This finding contrasts as well with results obtained by Bjorner and Jensen (2002) who investigate the consequences of a similar policy package in Denmark and obtain a positive effect of negotiated agreements on energy efficiency. Apart from institutional differences between the British and the Danish policy packages, the discrepancy might be owed to differences in the research design as these authors do not control for selection into negotiated agreements based on time-varying unobservables.

2006) nor our own shows any evidence that the targets negotiated under the CCA were very stringent, and the consistently low carbon prices in the UK ETS documented by Smith and Swierzbinski (2007) are in line with this as well. Rather than proving that a cap-and-trade system cannot establish meaningful prices for carbon, the UK experience demonstrates that a cap must put binding constraints on energy use to deliver real emissions reductions. It also serves as a reminder that the difficulties associated with this must not be underestimated. Aside from political factors, asymmetric information about abatement cost and the case-by-case nature of target negotiations with sector associations are likely to have left their mark on the final outcome.

Our study constitutes a first step towards building an evidence base that informs policy-makers about the impacts of climate change policies on industry. As more such policies are being implemented across countries, and as business microdata are becoming more abundant and easier to access, we expect that researchers will exploit the variation in policies and institutional settings to make important contributions to this evidence base. In the context of climate change policy in the UK, there are several issues that deserve attention in future research. First, it seems important to gain a better understanding of how plants achieved the substantial reductions in energy use that we measure. This will require gathering more qualitative information on the key drivers of energy conservation – be they technical, economic or managerial – which will facilitate the design of more sophisticated policy instruments. From a political economy point-of-view, a thorough analysis of the bargaining process in the setting of CCA targets and of compliance behaviour of individual CCA facilities will provide valuable insights regarding the design of negotiated agreements. Finally, given the long-term nature of climate change, an important open question is whether a moderate energy tax such as the CCL can stimulate much-needed innovation to bring about substantial carbon reductions in the future.

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Appendix - For Online Publication

A Estimation equations

Baseline model with linear sector and region trends Consider the level equation for energy consumption

$$y_{it} = const + \alpha T_{it} + S_i' \tilde{\beta}_S + t \cdot S_i' \beta_S + \eta_i + \xi_t + v_{it} \tag{A.1}$$

where T_{it} is the treatment indicator, S_i is a vector of sector dummies (region dummies are analogous), η_i is a plant fixed effect in the level of energy consumption, ξ_t is a year effect and v_{it} is the disturbance. Relabeling the year 2000 so that $t = 0$ and normalizing $\xi_0 = 0$ yields

$$y_{i0} = const + S_i' \tilde{\beta}_S + \eta_i + v_{i0}$$

and the level-t difference is given by

$$y_{it} - y_{i0} = \alpha T_{it} + t \cdot S_i' \beta_S + \xi_t + v_{it} - v_{i0}.$$

Similarly, we derive the pre-treatment difference

$$y_{i0} - y_{i-1} = S' \beta_S - \xi_{-1} + v_{i0} - v_{i-1}.$$

Based on this, we obtain the stacked equations used in the regression:

$$\begin{pmatrix} y_{i0} - y_{i-1} \\ y_{i1} - y_{i0} \\ y_{i2} - y_{i0} \\ y_{i3} - y_{i0} \\ y_{i4} - y_{i0} \end{pmatrix} = \alpha \begin{pmatrix} 0 \\ T_i \\ T_i \\ T_i \\ T_i \end{pmatrix} + \beta_S \begin{pmatrix} S_i \\ S_i \\ 2S_i \\ 3S_i \\ 4S_i \end{pmatrix} + \begin{pmatrix} \xi_{-1} \\ \xi_1 \\ \xi_2 \\ \xi_3 \\ \xi_4 \end{pmatrix} + \begin{pmatrix} v_{i0} - v_{i-1} \\ v_{i1} - v_{i0} \\ v_{i2} - v_{i0} \\ v_{i3} - v_{i0} \\ v_{i4} - v_{i0} \end{pmatrix} \tag{A.2}$$

Time-varying treatment effect Suppose now that the effect of the treatment is allowed to vary in each post-treatment period as in

$$y_{it} = const + \alpha_t D_{it} + S_i'\tilde{\beta}_S + t \cdot S_i'\beta_S + \eta_i + \xi_t + v_{it}. \tag{A.3}$$

Then the difference equation is given by

$$y_{it} - y_{i0} = \alpha_t D_{it} + t \cdot S_i'\beta_S + \xi_t + v_{it} - v_{i0}$$

and the stacked equations take the form

$$\begin{pmatrix} y_{i0} - y_{i-1} \\ y_{i1} - y_{i0} \\ y_{i2} - y_{i0} \\ y_{i3} - y_{i0} \\ y_{i4} - y_{i0} \end{pmatrix} = \begin{pmatrix} 0 \\ \alpha_1 T_i \\ \alpha_2 T_i \\ \alpha_3 T_i \\ \alpha_4 T_i \end{pmatrix} + \beta_S \begin{pmatrix} S_i \\ S_i \\ 2S_i \\ 3S_i \\ 4S_i \end{pmatrix} + \begin{pmatrix} \xi_{-1} \\ \xi_1 \\ \xi_2 \\ \xi_3 \\ \xi_4 \end{pmatrix} + \begin{pmatrix} v_{i0} - v_{i-1} \\ v_{i1} - v_{i0} \\ v_{i2} - v_{i0} \\ v_{i3} - v_{i0} \\ v_{i4} - v_{i0} \end{pmatrix}. \tag{A.4}$$

Unobserved trends in the treatment group Suppose there are unobserved trends that differ systematically between treated and non-treated plants, i.e.

$$y_{it} = const + \alpha T_{it} + S_i'\tilde{\beta}_S + t \cdot S_i'\beta_S + \eta_i + \delta t \cdot T_i + \xi_t + v_{it} \tag{A.5}$$

The stacked differenced equations take the form:

$$\begin{pmatrix} y_{i0} - y_{i-1} \\ y_{i1} - y_{i0} \\ y_{i2} - y_{i0} \\ y_{i3} - y_{i0} \\ y_{i4} - y_{i0} \end{pmatrix} = \alpha \begin{pmatrix} 0 \\ T_i \\ T_i \\ T_i \\ T_i \end{pmatrix} + \beta_S \begin{pmatrix} S_i \\ S_i \\ 2S_i \\ 3S_i \\ 4S_i \end{pmatrix} + \delta \begin{pmatrix} T_i \\ T_i \\ 2T_i \\ 3T_i \\ 4T_i \end{pmatrix} + \begin{pmatrix} \xi_{-1} \\ \xi_1 \\ \xi_2 \\ \xi_3 \\ \xi_4 \end{pmatrix} + \begin{pmatrix} v_{i0} - v_{i-1} \\ v_{i1} - v_{i0} \\ v_{i2} - v_{i0} \\ v_{i3} - v_{i0} \\ v_{i4} - v_{i0} \end{pmatrix}. \tag{A.6}$$

In the IV estimation, we use Z_i and tZ_i as instrumental variables for T_i and tT_i, respectively.

B IV estimation with an imperfectly observed eligibility rule

The IV estimator of the effect of a treatment D_i on an outcome y_i using instrument z_i is given by

$$\hat{\theta} = \frac{E\left[y_i\,|z_i = 1\right] - E\left[y_i\,|z_i = 0\right]}{E\left[D_i\,|z_i = 1\right] - E\left[D_i\,|z_i = 0\right]}. \tag{B.1}$$

Since the instrument is based on an eligibility rule we use that

$$E\left[D_i\,|z_i = 0\right] = \Pr\left[D_i = 1|z_i = 0\right] = 0. \tag{B.2}$$

A well-known consequence is that $\hat{\theta}$ recovers the average treatment effect on the treated (ATT)

$$\hat{\theta} = \frac{E\left[\Delta y_i D_i\,|z_i = 1\right]}{E\left[D_i\,|z_i = 1\right]} = \frac{E\left[\Delta y_i\,|z_i = 1, D_i = 1\right]\Pr\left[D_i = 1\,|z_i = 1\right]}{\Pr\left[D_i = 1\,|z_i = 1\right]} = E\left[\Delta y_i\,|D_i = 1\right] \tag{B.3}$$

where we have used the notation $\Delta y_i \equiv y_{1i} - y_{0i}$ and the independence of z_i and y_i.

In our application, we only observe an imperfect measure of the eligibility rule, $\tilde{z}$, which equals zero for some eligible firms. For example, some firms emit pollutants covered under PPC legislation but are not contained in the EPER database because the quantities emitted are below the reporting threshold. In contrast, whenever $\tilde{z} = 1$ we know for sure that a firm is eligible, i.e.

$$\Pr\left(z_i = 0\,|\tilde{z}_i = 1\,\right) = 0. \tag{B.4}$$

Consider the IV estimator with the imperfect instrument $\tilde{z}_i$

$$\tilde{\theta} = \frac{E\left[y_i\,|\tilde{z}_i = 1\right] - E\left[y_i\,|\tilde{z}_i = 0\right]}{E\left[D_i\,|\tilde{z}_i = 1\right] - E\left[D_i\,|\tilde{z}_i = 0\right]} \equiv \frac{\tilde{\rho}}{\tilde{\delta}} \tag{B.5}$$

The denominator of this expression can be written as

$$\tilde{\delta} = \Pr\left(D_i = 1|\tilde{z}_i = 1\right) - \Pr\left(D_i = 1|\tilde{z}_i = 0\right) \tag{B.6}$$

The numerator corresponds to the reduced form regression of y on z and can be written as

$$\tilde{\rho} = E\left\{\Delta y_i D_i \,|\tilde{z}_i = 1\right\} - E\left\{\Delta y_i D_i \,|\tilde{z}_i = 0\right\}. \tag{B.7}$$

Note that from (B.4) we get for the first term in (B.7):

$$E\left\{\Delta y_i D_i \,|\tilde{z}_i = 1\right\} = E\left\{\Delta y_i D_i \,|\tilde{z}_i = 1, z_i = 1\right\} = E\left\{\Delta y_i \,|\tilde{z}_i = 1, z_i = 1, D_i = 1\right\} P\left\{D_i = 1 \,|\tilde{z}_i = 1\right\} \tag{B.8}$$

and for the second term[1]

$$E\left\{\Delta y_i D_i \,|\tilde{z}_i = 0\right\} = E\left\{\Delta y_i \,|\tilde{z}_i = 0, z_i = 1, D_i = 1\right\} \Pr\left\{D_i = 1 \,|\tilde{z}_i = 0\right\} \tag{B.9}$$

To simplify notation, let $E_n \equiv E[\Delta y_i | D_i = 1, \tilde{z}_i = n, z_i = 1]$ and $p_n \equiv \Pr(D_i = 1 \,|\tilde{z}_i = n)$ for $n \in \{0,1\}$. This yields

$$\tilde{\theta} = \frac{E_1 p_1 - E_0 p_0}{p_1 - p_0}. \tag{B.10}$$

How does this estimator relate to the ATT $\hat{\theta}$? Denoting by $\bar{E} = E_0 + \lambda(E_1 - E_0)$ where $\lambda \equiv \Pr(\tilde{z}_i = 1 | D_i = 1, z_i = 1) = \Pr(\tilde{z}_i = 1 | D_i = 1)$ we obtain

$$\begin{aligned}
\tilde{\theta} &\gtreqless \hat{\theta} \\
\frac{E_1 p_1 - E_0 p_0}{p_1 - p_0} &\gtreqless \frac{\bar{E}(p_1 - p_0)}{p_1 - p_0} \\
\frac{E_1 p_1 - E_0 p_0}{p_1 - p_0} &\gtreqless \frac{E_0 p_1 + \lambda p_1 (E_1 - E_0) - E_0 p_0 - \lambda p_0 (E_1 - E_0)}{p_1 - p_0} \\
\frac{p_1 (E_1 - E_0)}{p_1 - p_0} &\gtreqless \frac{\lambda (p_1 - p_0)(E_1 - E_0)}{p_1 - p_0} \\
\frac{E_1 - E_0}{p_1 - p_0}\left[\lambda p_0 + (1 - \lambda) p_1\right] &\gtreqless 0 \\
\frac{E_1 - E_0}{p_1 - p_0} &\gtreqless 0
\end{aligned}$$

since the term in brackets is the linear combination of two positive probabilities and hence

[1] Write $E\left\{\Delta y_i D_i \,|\tilde{z}_i = 0\right\} = E\left\{\Delta y_i D_i \,|z_i = 1, \tilde{z}_i = 0\right\} \Pr\left\{z_i = 1 \,|\tilde{z}_i = 0\right\} + E\left\{\Delta y_i D_i \,|z_i = 0, \tilde{z}_i = 0\right\} \Pr\left\{z_i = 0 \,|\tilde{z}_i = 0\right\}$. The second term of this expression, $E\left\{\Delta y_i D_i \,|z_i = 0, \tilde{z}_i = 0\right\} = E\left\{\Delta y_i \,|D_i = 1,\, z = 0,\, \tilde{z}_i = 0\right\} \Pr\left\{D_i = 1 \,|z_i = 0,\, \tilde{z}_i = 0\right\}$ equals zero because of condition (B.2).

positive. In our application, $p_1 - p_0 > 0$ so that the condition implies

$$\tilde{\theta} \gtreqless \hat{\theta} \Leftrightarrow E[\Delta y_i | D_i = 1, \tilde{z}_i = 1, z_i = 1] \gtreqless E[\Delta y_i | D_i = 1, \tilde{z}_i = 0, z_i = 1].$$

This result is intuitive. If the average treatment effect on firms that we observe as eligible is higher than among those that are erroneously identified as not eligible, then we overestimate the ATT and vice versa.

C Additional Tables and Figures

Table C.1: Descriptive statistics - ARD-QFI joint sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables	**Mean**	**SD**	**SD, between**	**SD,within**	**p10**	**p90**	**Observations**
Electricity (El)	21,571.08	59,485.11	61,077.61	10,696.47	797.26	46,153.84	1,809
Electricity expenditures (ElE)	620.98	1,481.61	1,531.79	219.01	36.20	1,434.32	1,809
Liquid fuels (Li)	0.80	9.24	8.85	1.74	0.00	0.02	1,809
Liquid fuels expenditures (LiE)	37.90	344.41	331.48	51.35	0.00	4.21	1,809
Gas (Gas)	39,105.85	128,936.70	139,801.40	25,588.45	0.00	80,934.28	1,809
Gas expenditures (GasE)	304.22	963.13	1,076.44	191.95	0.00	638.93	1,809
Gas share in Gas+El (Gas/(Gas+El))	0.26	0.21	0.19	0.06	0.00	0.54	1,809
Gas expenditure share (GasE/ (GasE+ElE))	0.47	0.30	0.29	0.08	0.00	0.82	1,809
Gas share (Gas/kWh)							
Solid fuels (So)	1.12	7.17	10.80	1.83	0.03	2.01	651
Solid fuels expenditures (SoE)	171.25	808.94	1,179.95	238.70	6.21	356.59	651
Solid fuels share (So/kWh)							
Total kWh (kWh)	71,446.32	213,729.90	255,849.80	34,441.89	2,149.22	152,217.40	1,809
Total kWh expenditures (kWhE)	1,020.18	2,497.42	2,960.57	334.69	56.63	2,426.99	1,809
Total kWh over GO (kWh/GO)	1,261.85	2,287.06	2,084.05	333.92	122.87	3,004.09	1,809
CO2 (CO2)	27,313.31	76,128.18	88,373.85	11,905.70	984.83	61,004.94	1,809
CO2 intensity of energy use (CO2/kWh)	0.45	0.13	0.13	0.04	0.30	0.66	1,809
CO2 over GO (CO2/GO)	468.94	676.40	631.79	107.12	56.57	1,137.28	1,809

Notes: Descriptive statistics for QFI variables in the joint sample of firms with lnGO and lnEl not missing, pooled for 1999-2004. All the expenditure variables are in thousands of pounds. Total kWh, Gas and El are in thousands of kWh. So and Li are in thousands of tonnes. The CO2 variable measures total CO2 emissions in thousands of tonnes based on fuel use (the conversion factors are from the Entech Utility Service Bureau, for more details see Martin, 2006).

Table C.2: Differences in growth rates pre-treatment

	(1) CCL=0	(2) CCL=1	(3) Diff.	(4) NEPER=0	(5) NEPER=1	(6) Diff.
A. ARD variables						
Energy share in gross output	-0.005	-0.000	-	0.005	-0.001	-
ln(EE/GO)	697	3,851		243	4,305	
Energy share in var. costs	-0.008	-0.017	-	-0.009	-0.016	-
ln(EE/VCost)	697	3,851		243	4,305	
Energy expenditure	0.030	0.025	-	0.037	0.025	-
ln(EE)	697	3,851		243	4,305	
Real gross output	0.034	0.025	-	0.031	0.026	-
ln(Real GO)	697	3,851		243	4,305	
Employment	-0.017	-0.022	-	-0.017	-0.022	-
ln(L)	689	3,827		243	4,305	
Capital stock	0.029	0.017	*	0.020	0.019	-
ln(K)	697	3,830		243	4,284	
Materials	0.042	0.046	-	0.057	0.044	-
ln(M)	697	3,851		243	4,305	
Age	0.042	0.046	-	0.057	0.044	-
	697	3,851		243	4,305	
B. QFI variables						
Electricity	0.012	-0.008	-	-0.003	-0.002	-
ln(El)	149	368		52	465	
Gas	0.071	0.047	-	0.159	0.044	-
ln(Gas)	123	298		38	383	
Gas share in gas + electricity	0.003	0.014	-	0.014	0.010	-
(Gas/(Gas+El))	149	368		52	465	
Gas share	-0.019	0.003	-	-0.004	-0.003	-
(Gas/kWh)	149	368		52	465	
Solid fuels	-0.033	0.013	-	-0.279	0.050	*
ln(So)	57	130		29	158	
Solid fuels share	0.000	0.001	-	0.002	0.001	-
(So/kWh)	149	368		52	465	
Total kWh	-0.029	0.004	-	-0.003	-0.005	-
ln(kWh)	149	368		52	465	
CO2	-0.009	0.000	-	0.001	-0.003	-
ln(CO2)	149	368		52	465	

Notes: Summary statistics for the difference in growth rates between year 1999 and 2000 by CCL and NEPER status. For each variable, we report the mean and the number of observations in the row below the variable mean. Columns 3 and 6 report significance levels of a t-test of differences in group means with unequal variance. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Figure C.1: Trends in outcome variables by treatment status

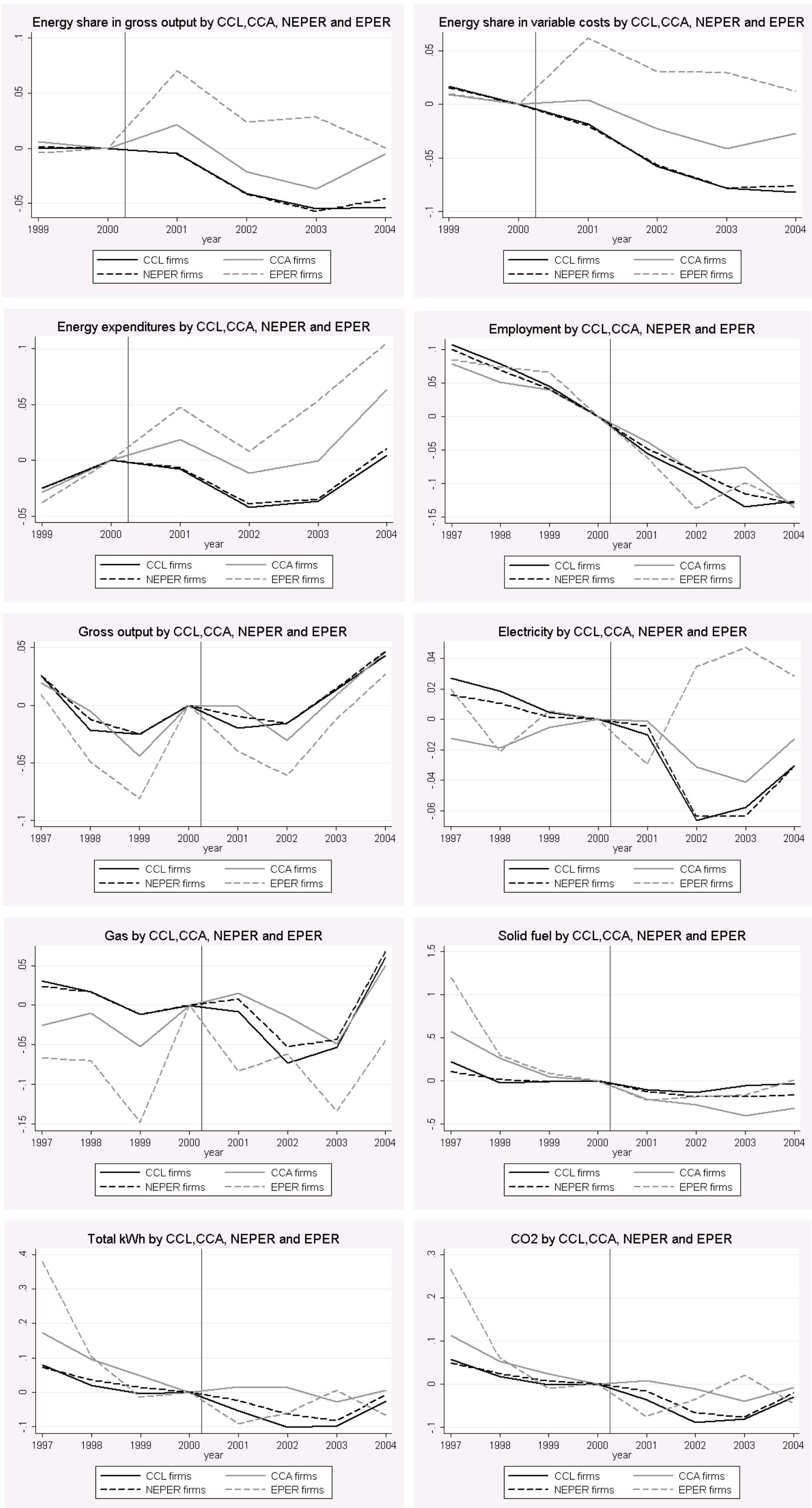

Table C.3: CCL impact in a balanced sample - ARD

Dependent variables	Year	(1) OLS	(2) RF	(3) IV	(4) Obs./ Plants
Energy share in gross output	2001	-0.014	-0.036	-0.132	6,855
		(0.019)	(0.030)	(0.103)	1,509
	2002	-0.021	-0.073**	-0.253**	
Δln(EE/GO)		(0.022)	(0.034)	(0.124)	
	2003	-0.024	-0.057	-0.250	
		(0.026)	(0.038)	(0.160)	
	2004	-0.047	-0.064	-0.313	
		(0.029)	(0.048)	(0.218)	
Energy share in var. costs	2001	-0.015	-0.037	-0.139	6,855
		(0.019)	(0.030)	(0.104)	1,509
	2002	-0.028	-0.092***	-0.323**	
Δln(EE/VCost)		(0.021)	(0.033)	(0.126)	
	2003	-0.035	-0.084**	-0.358**	
		(0.025)	(0.038)	(0.164)	
	2004	-0.067**	-0.087*	-0.428*	
		(0.029)	(0.047)	(0.220)	
	2001	-0.021	-0.015	-0.052	6,855
Energy expenditure		(0.019)	(0.027)	(0.092)	1,509
	2002	-0.023	-0.048	-0.176*	
Δln(EE)		(0.021)	(0.029)	(0.106)	
	2003	-0.041*	-0.042	-0.206	
		(0.024)	(0.032)	(0.137)	
	2004	-0.068**	-0.077*	-0.349*	
		(0.027)	(0.043)	(0.196)	
	2001	-0.008	0.022	0.079	6,866
Real gross output		(0.012)	(0.019)	(0.065)	1,513
	2002	-0.002	0.024	0.076	
Δln(Real GO)		(0.017)	(0.024)	(0.085)	
	2003	-0.017	0.014	0.044	
		(0.021)	(0.028)	(0.115)	
	2004	-0.023	-0.013	-0.038	
		(0.026)	(0.037)	(0.164)	
	2001	0.000	-0.006	-0.015	6,866
Employment		(0.012)	(0.016)	(0.054)	1,513
	2002	0.004	0.005	0.015	
Δln(L)		(0.016)	(0.020)	(0.073)	
	2003	-0.012	0.013	0.039	
		(0.020)	(0.026)	(0.108)	
	2004	0.000	-0.003	-0.001	
		(0.025)	(0.034)	(0.152)	
Total factor productivity	2001	-0.002	0.014	0.046	6,857
		(0.008)	(0.013)	(0.043)	1,512
	2002	-0.003	-0.002	-0.009	
Δln(GO)		(0.010)	(0.017)	(0.059)	
	2003	-0.006	-0.012	-0.049	
		(0.012)	(0.016)	(0.068)	
	2004	-0.017	-0.017	-0.081	
		(0.013)	(0.019)	(0.087)	

Notes: Column 1 displays the OLS coefficient on the treatment variable interacted with dummies for post-treatement years. Column 2 displays the OLS coefficient on the instrumental variable (and year interactions) in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable (and year interactions). Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. All regressions include age, age squared, and controls for year, region and 3-digit industry effects. The total factor productivity regressions also control for labor, capital stock, and for expenditures on materials and 28 energy. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Table C.4: CCL impact in balanced sample - QFI

		(1)	(2)	(3)	(4)
Dependent variables	**Year**	**OLS**	**RF**	**IV**	**Obs./ Plants**
	2001	-0.005	-0.024	-0.069	2,748
Electricity		(0.022)	(0.037)	(0.106)	480
	2002	-0.025	-0.074*	-0.218*	
Δln(El)		(0.027)	(0.041)	(0.126)	
	2003	-0.046	-0.118**	-0.357**	
		(0.035)	(0.047)	(0.170)	
	2004	-0.077*	-0.139**	-0.448**	
		(0.045)	(0.057)	(0.214)	
	2001	-0.026	0.128***	0.366**	2,086
Gas		(0.043)	(0.049)	(0.169)	360
	2002	-0.102**	-0.040	-0.100	
Δln(Gas)		(0.050)	(0.059)	(0.147)	
	2003	-0.126**	-0.063	-0.184	
		(0.060)	(0.072)	(0.183)	
	2004	-0.074	-0.050	-0.178	
		(0.077)	(0.089)	(0.247)	
	2001	-0.022**	0.042	0.123	2,748
Gas share		(0.010)	(0.030)	(0.094)	480
	2002	-0.043***	0.028	0.081	
Δ(Gas/kWh)		(0.012)	(0.031)	(0.089)	
	2003	-0.031**	0.025	0.077	
		(0.014)	(0.030)	(0.091)	
	2004	-0.018	0.031	0.098	
		(0.017)	(0.034)	(0.109)	
	2001	-0.002	-0.009	-0.027	2,761
Solid fuels share		(0.004)	(0.011)	(0.030)	482
	2002	0.004	0.014	0.039	
Δ(So/kWh)		(0.004)	(0.010)	(0.028)	
	2003	0.006	0.011	0.034	
		(0.005)	(0.012)	(0.036)	
	2004	0.004	0.027*	0.084	
		(0.006)	(0.015)	(0.052)	
	2001	-0.074***	0.037	0.101	2,761
Total kWh		(0.028)	(0.052)	(0.152)	482
	2002	-0.146***	-0.037	-0.102	
Δln(kWh)		(0.038)	(0.053)	(0.138)	
	2003	-0.119**	-0.099	-0.276	
		(0.046)	(0.067)	(0.185)	
	2004	-0.108*	0.002	-0.018	
		(0.056)	(0.071)	(0.208)	
	2001	-0.049**	0.020	0.054	2,761
CO2 emissions		(0.022)	(0.042)	(0.121)	482
	2002	-0.095***	-0.041	-0.117	
Δln(CO2)		(0.028)	(0.040)	(0.108)	
	2003	-0.086**	-0.104*	-0.298*	
		(0.036)	(0.055)	(0.161)	
	2004	-0.096**	-0.041	-0.148	
		(0.045)	(0.054)	(0.158)	

Notes: Column 1 displays the OLS coefficient on the treatment variable interacted with dummies for post-treatment years. Column 2 displays the OLS coefficient on the instrumental variable (and year interactions) in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable (and year interactions). Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. All regressions include age, age squared, and control for year, region and 3-digit industry effects. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Table C.5: Effects of CCL and NEPER firms trend - ARD sample

Dependent variables	Year	(1) OLS	(2) RF	(3) IV	(4) Obs./ Plants
Energy share in gross output	2001	-0.020	-0.073**	-0.163**	16,917
		(0.010)	(0.030)	(0.070)	6,901
	2002	-0.010	-0.050	-0.090	
Δln(EE/GO)		(0.020)	(0.050)	(0.140)	
	2003	-0.010	-0.040	-0.030	
		(0.020)	(0.070)	(0.200)	
	2004	-0.041*	-0.060	-0.050	
		(0.020)	(0.090)	(0.290)	
	NEPER *year diff	-0.015*	0.000	-0.010	
		(0.010)	(0.020)	(0.020)	
	2001	-0.020	-0.040	-0.092*	16,917
Energy expenditure		(0.010)	(0.030)	(0.060)	6,901
	2002	-0.010	-0.020	-0.050	
Δln(EE)		(0.020)	(0.050)	(0.120)	
	2003	-0.010	-0.030	-0.090	
		(0.020)	(0.060)	(0.180)	
	2004	-0.030	-0.080	-0.220	
		(0.020)	(0.080)	(0.260)	
	NEPER *year diff	-0.010	0.010	0.000	
		(0.010)	(0.020)	(0.020)	
	2001	0.000	0.030	0.070	16,917
Real gross output		(0.010)	(0.020)	(0.050)	6,901
	2002	0.000	0.030	0.040	
Δln(Real GO)		(0.010)	(0.040)	(0.100)	
	2003	0.000	0.010	-0.060	
		(0.020)	(0.050)	(0.160)	
	2004	0.010	-0.020	-0.170	
		(0.020)	(0.070)	(0.230)	
	NEPER *year diff	0.010	0.010	0.010	
		(0.010)	(0.020)	(0.010)	
	2001	0.010	0.030	0.060	16,917
Employment		(0.010)	(0.020)	(0.050)	6,901
	2002	0.000	0.030	0.050	
Δln(L)		(0.010)	(0.040)	(0.100)	
	2003	0.000	0.040	0.050	
		(0.020)	(0.060)	(0.170)	
	2004	0.020	0.000	-0.080	
		(0.020)	(0.080)	(0.240)	
	NEPER *year diff	0.010	0.000	0.010	
		(0.010)	(0.020)	(0.020)	

Notes: Column 1 displays the OLS coefficient on the treatment variable interacted with dummies for post-treatment years. Column 2 displays the OLS coefficient on the instrumental variable (and year interactions) in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable (and year interactions). Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. NEPER is a dummy variable that equals one if a facility is not on the EPER list. All regressions include a time-invariant eligibility dummy interacted with year differences (NEPER*year difference), age, age squared, and control for year, region and 3-digit industry effects. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Table C.6: Effects of CCL and NEPER firms trend - QFI sample

		(1)	(2)	(3)	(4)
Dependent variables	**Year**	**OLS**	**RF**	**IV**	**Obs./ Plants**
	2001	-0.020	-0.020	-0.050	4,587
Electricity		(0.020)	(0.040)	(0.100)	1,079
	2002	-0.030	-0.112**	-0.350**	
Δln(El)		(0.030)	(0.050)	(0.170)	
	2003	-0.030	-0.144**	-0.453*	
		(0.040)	(0.070)	(0.250)	
	2004	-0.040	-0.130	-0.450	
		(0.050)	(0.090)	(0.320)	
	NEPER *year diff	-0.022**	0.010	0.000	
		(0.010)	(0.020)	(0.010)	
	2001	0.010	0.123**	0.320**	3,748
Gas		(0.040)	(0.050)	(0.150)	908
	2002	-0.095**	0.020	0.040	
Δln(Gas)		(0.050)	(0.080)	(0.220)	
	2003	-0.090	0.050	0.110	
		(0.060)	(0.110)	(0.370)	
	2004	-0.100	0.120	0.300	
		(0.080)	(0.150)	(0.590)	
	NEPER *year diff	0.010	-0.010	-0.010	
		(0.020)	(0.030)	(0.020)	
	2001	-0.080***	0.020	0.050	4,605
Total kWh		(0.030)	(0.050)	(0.130)	1,082
	2002	-0.141***	-0.040	-0.130	
Δln(kWh)		(0.040)	(0.080)	(0.220)	
	2003	-0.125***	-0.150*	-0.460	
		(0.040)	(0.090)	(0.290)	
	2004	-0.090	-0.020	-0.140	
		(0.060)	(0.120)	(0.380)	
	NEPER *year diff	0.010	0.010	0.010	
		(0.010)	(0.020)	(0.020)	
	2001	-0.055***	0.010	0.030	4,605
CO2 emissions		(0.020)	(0.040)	(0.100)	1,082
	2002	-0.093***	-0.060	-0.210	
Δln(CO2)		(0.030)	(0.050)	(0.150)	
	2003	-0.079**	-0.156**	-0.487**	
		(0.040)	(0.070)	(0.240)	
	2004	-0.070	-0.060	-0.270	
		(0.050)	(0.080)	(0.290)	
	NEPER *year diff	-0.010	0.010	0.010	
		(0.010)	(0.020)	(0.010)	

Notes: Column 1 displays the OLS coefficient on the treatment variable interacted with dummies for post-treatment years. Column 2 displays the OLS coefficient on the instrumental variable (and year interactions) in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable (and year interactions). Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. NEPER is a dummy variable that equals one if a facility is not on the EPER list. All regressions include a time-invariant eligibility dummy interacted with year differences (NEPER*year difference), age, age squared, and control for year, region and 3-digit industry effects. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Table C.7: CCL impact in a common support sample

Dependent variables	(1) OLS	(2) RF	(3) IV	(4) Obs./ Plants
A. ARD variables				
Energy share in gross output	-0.024*	-0.069***	-0.214***	15,614
Δln(EE/GO)	(0.013)	(0.022)	(0.071)	6,300
Energy share in var. costs	-0.028**	-0.077***	-0.240***	15,614
Δln(EE/VCost)	(0.013)	(0.022)	(0.071)	6,300
Energy expenditure	-0.020	-0.035*	-0.110*	15,614
Δln(EE)	(0.013)	(0.019)	(0.061)	6,300
Employment	0.011	0.028	0.089*	15,614
Δln(L)	(0.011)	(0.017)	(0.054)	6,300
Real gross output	0.004	0.033*	0.104*	15,614
Δln(Real GO)	(0.011)	(0.017)	(0.054)	6,300
Total factor productivity	0.001	0.002	0.006	15,594
Δln(GO)~inputs	(0.006)	(0.011)	(0.033)	6,296
B. QFI variables				
Electricity	-0.033	-0.069**	-0.224**	3,327
Δln(El)	(0.023)	(0.033)	(0.113)	592
Gas	-0.057	0.063	0.203	2,739
Δln(Gas)	(0.038)	(0.047)	(0.169)	510
Gas share in gas + electricity	-0.025***	0.033	0.106	3,327
Δ(Gas/(Gas+El))	(0.009)	(0.022)	(0.074)	592
Gas share	-0.024**	0.020	0.064	3,333
Δ(Gas/kWh)	(0.012)	(0.017)	(0.057)	594
Solid fuels	0.119	0.162	0.686	1,112
Δln(So)	(0.095)	(0.160)	(0.647)	256
Solid fuels share	-0.004	0.005	0.015	3,335
Δ(So/kWh)	(0.004)	(0.008)	(0.026)	594
Total kWh	-0.113***	-0.008	-0.027	3,335
Δln(kWh)	(0.028)	(0.039)	(0.118)	594
CO2	-0.079***	-0.029	-0.092	3,335
Δln(CO2)	(0.022)	(0.032)	(0.096)	594

Notes: Column 1 displays the OLS coefficient on the treatment variable, column 2 displays the OLS coefficient on the instrumental variable in the reduced form, and column 3 displays the 2SLS coefficient on the treatment variable. Column 4 reports the number of observations and plants. Dependent variables are first-differenced from 1997 until 2000 and differenced at various intervals thereafter. All regressions include age, age squared, year , 3-digit industry code and region-by-year dummies. The TFP regressions also control for labor, capital stock, and for expenditures on materials and energy. Robust standard errors are in parenthesis. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

Table C.8: Additional exit regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All		**Energy intensity**				**Trade intensity**			
			low		*high*		*low*		*high*	
	RF	**FS**	**RF**	**FS**	**RF**	**FS**	**RF**	**FS**	**RF**	**FS**
CCL/SMALL * $I_{\{t>2000\}}$	0.000	0.022***	-0.001	0.017***	0.001	0.032***	0.001	0.029***	-0.001	0.022***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
SMALL	0.033***	-0.001***	0.031***	-0.001***	0.036***	-0.001***	0.034***	-0.001***	0.032***	-0.001***
	(0.001)	(0.000)	(0.002)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.002)	(0.000)
Observations	770,991	770,991	348,834	348,834	416,157	416,157	380,288	380,288	293,906	293,906

Notes: The table reports results from reduced-form (RS) and first-stage (FS) regressions of the exit equation when 2-digit sector dummies are included. Columns 1 and 2 correspond to column 2 and 3 in Table 9. The remaining columns report the results from regressions after splitting the sample according to the sector's energy intensity (columns 3 to 6) and trade intensity (columns 7 through 10), as explained in Section 6.1. SMALL is a dummy indicating that employment at the plant was below the median in 1997. All regressions including year dummies, age and age squared. The sample period ranges from 1998 to 2004. Standard errors are clustered at the local unit level. Asterisks indicate statistical significance at 10% (*), at 5% (**) and at 1% (***).

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